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Semantics of the Gendered Body in the IOC's Medical Commission between 1967 and 1972

Par

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Résumé

Ce mémoire porte sur les tests de féminité dans le sport de haut niveau. Plus particulièrement, les tests qui ont été menés par le Comité Olympique International (COI). Cette étude débute avec un survol historique des classifications du corps en sciences biomédicales et en sciences sociales, ainsi que de la place des femmes dans le sport et des tests de féminités. Ensuite, à travers une analyse de contenu des procès-verbaux, correspondances, et études présentées à la Commission Médicale du COI entre 1967 et 1972, cette recherche relève six catégories de discours sur le corps. Les résultats de cette analyse se déploient en deux temps : d'abord les discours à propos du corps et les différences de sexe et ensuite le rapport entre le corps et les membres de la Commission Médicale. Les trois discours relevés par rapport au corps sont : « la nature polymorphe du corps», « le corps comme dimorphique », et « le corps anormal ». Les discours par rapport à la relation entre corps et la commission médicale sont : « le corps comme objet scientifique », « le corps comme catégorie abstraite », et « le corps comme objet de préoccupation éthique ».

Mots-clés : tests de féminité, CIO, Olympisme, sportives, genre, sexe, corps, epistémologie féministe, analyse de contenu.

Abstract

This thesis focuses on gender testing in high level sport. More specifically, those conducted by the International Olympic Committee (IOC). I begin with a historical contextualisation of the various classifications of the body biomedical and social sciences have put forward. Then, through a content analysis of the minutes, correspondences, and studies read by the Medical Commission between 1967 and 1972, I highlight six categories of discourse on the body. These are divided into two types of categories: first the ways in which the body and sex differences are talked about; then, the relationship between the body and the members of the Medical Commission. The three types of discourses relating to the body are: “the polymorphic nature of the body", "the body as dimorphic", and "the abnormal body". Discourses on the relationship between the abnormal body are : "the body as scientific object", “the body as abstract category”, and "the body as an object of ethical concern ".

Keywords : gender-testing, IOC, sport, gender, sex, body, women athletes, Olympics, feminist epistemology, content analysis.

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Abbreviation List

AIS : Androgen Insensitivity Syndrome

CAH : Congenital Adrenal Hyperplasia

CAS : Court of Arbitration for Sport

DSD : Differences in Sexual Development

FSH : Follicle-Stimulating Hormone

IAAF : International Association of Athletics Federation

IF : International Federation

IOC : International Olympic Committee

LBM : Lean Body Mass

MC : Medical Commission

NOC : National Olympic Committee

OCOG : Organizing Committee of the Olympic Games

OG : Olympic Games

TUA : Therapeutic Use Authorization

WADA : World Anti-Doping Agency

WWI: First World War

WWII: Second World War

To the Strong Women of my life,

And in memory of my beloved father

Acknowledgements

Since I don't belong here, I feel I owe many people many things. First, I thank my family and friends. Thank you to François and Raymonde Donato, my grandparents, for their material and emotional support. My parents, Sonia Filion and late François Donato, for, well, literally everything. My thanks also go to my angel sister, Anne Filion-Donato, for her immense if unassuming wisdom; to my supportive and vivacious aunt and uncles, Christiane Filion, Bruno Amoros, and Jacques Donato, for their very real material support. Thank you for providing the best safe-haven a student could ask for on the last two months of writing this thesis. I extend my thanks to my chosen family, Will Burton and Sarah Woolf, for their deep and creative patience with my idiosyncrasies and their unfailing willingness to gently challenge me; to my dear friends, in no particular order, Jonas Wiegert, who provided inestimable help in the IOC archives, Justine McLellan, Myriam Zaidi, Victoria Pilger, Philémon Gravel, Claire Morrison, Jonathan Pedneault, Bernardo Bianchi, and Till Gathmann, for the endless sources of inspiration, joy, and solace I found in and with them. Thank you to my friends Dr. Andreanne Cromp and Dr. Thomas Quail for sharing their scientific and practical knowledge for the benefit of this research. My loving thanks also go to Julien Mendoza for the vitalizing conversations, quotidian belly-hurting laughs, fervent support, and so much more.

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TL;DR, and to unfaithfully quote G. Bataille: this was, as is everything, a group effort; *ce que j'ai pensé, je ne l'ai pas pensé seule*.

Introduction

In 2009, South African runner Semenya Caster wins the 800m race at the Berlin world championships with 8 seconds on her best time. Before confirming her victory, and despite having weened out gender testing from its practices since 1992, the International Athletic Federation (IAAF) has Caster go through a battery of tests to determine her "true" sex. This is the first time an athlete is tested since the Atlanta Olympic Games of 1996 and the first time in recent memory these tests generate such a controversy — the last controversy of this kind being that of Spanish runner María José Martínez-Patiño in 1985.

Science and Technique Studies (STS) loves controversies. Usually, however, the controversies it studies are scientific ones (e.g. Brower and Pointcarré). Gender testing, or what Caster had to go through in 2009, cannot be said to be a scientific controversy, or at least it does not appear to be at first sight. Apart from being first and foremost a social controversy, it also opposes different scientific fields, rather than factions of the same field. That is, it opposes a conception of sex/gender that belongs to the social sciences and one which belong to the biomedical sciences. Each has a definition of gender/sex that seems diametrically opposed to the other. The first suggests that the biological constitution of an individual (his or her sex) has no incidence on the roles, horizons, and capabilities of that individual. Whereas the second seems to suggests that it does. This is where gender tests come in. The rationale behind them is that if men possess some biological advantage on women when it comes to physical performance, keeping a "level playing field" means that sport events ought to be sex-segregated. This sex-segregation is then insured by the tests.

This, however, is not the only tension these tests reveal. Indeed, though women athletes have been subjugated to gender (or femininity tests) since 1936, there have been several iterations of these tests through time, something that expresses, if not a controversy within the biomedical sciences—since the medical consensus of the time, at each iteration, is generally in line with the methods used—at least some profound ambiguity and incomprehension regarding what differentiates the so called two sexes.

In an effort to analyse these ambiguities within the biomedical discipline and the controversies between scientific disciplines, this Master's thesis is devoted to the topic of gender testing in the International Olympic Committee (IOC). More specifically, I present a content analysis of the Meeting Minutes and correspondences of the Medical Commission (MC) of the IOC between 1967 and 1972. This analysis is supplemented by a historical and conceptual contextualisation taking shape in six chapters. In Chapter 1, I present a literature review of the various ways in which sex and gender have been conceptualized by the social and biomedical sciences. In Chapter 2, I give an overview of the history of women in sports as well as that of the gender tests. In Chapter 3, I present my epistemological standpoint and theoretical framework. My research strategy and detailed description of the content used for the analysis are discussed in Chapter 4. These contents include the Meeting Minutes of the MC, letters exchanged within the IOC, and medical reports available to the Medical Commission in the target years. In Chapter 5, I present the analysis made out of the Meeting Minutes. This analysis reveals six categories that fit into two groups. The first group is one about the ways in which the body and sex differences are discussed. The second covers the ways in which the relationship between the body and the members of the Medical Commission is talked about. In the first group, we find three types of discourses on the body: "the polymorphic nature of the body", "the body as dimorphic", and "the abnormal body". In the second one, the relationship between the abnormal body also unfolds into three types: "the body as scientific object", "the body as abstracted category", and "the body as object of ethical concern ". I end with a chapter discussing those results.

Chapter 1 – Sexual Dimorphism or Bicategorization?

Sexual dimorphism describes the condition, among a specie, of being divided into two sexual groups, called male and female. This division is based on the observed variation of morphological traits between individuals of the same species. These traits, such as size, colour, pilosity, etc., are called secondary sexual characteristics. Male ducks are, for example, usually more colourful than female ones and, in mammals, most males are larger than the females. Because this division is based on a wide corpus of observations, it seems difficult to question.

Social scientists, however, have pointed out that the concept of sexual dimorphism is limited. First, though it encompasses multiple types of differences, it says nothing about the kind or degree of differences it focuses on. Second, there is a well-documented inversion of these morphological variations across species. For example, bearing in mind the dimorphic sizes in mammals just mentioned, it is worth mentioning that, in most insects, the female is in fact larger than the male. When one thinks of the concept of sexual dimorphism, therefore, this cross-species variation is oft evacuated. Third, this concept also does not address the existence of quantitatively more significant extra-sexual variations within species.

These problems seem to indicate that the notion of sexual dimorphism may emphasise certain differences over others and therefore reproduce cultural biases more than merely describe natural differences. This line of argument, proposed by the social sciences, is captured by the notion of *bicategorization*. The idea is that this particular type of binary organisation is the result of the superimposition of (human) categories onto natural phenomena or data interpretation. This will be further discussed in the present chapter.

To sort out the distinction between sexual dimorphism and sexual bicategorization, as well as present some of the key concepts of use in this thesis, this chapter traces the ways in which and places where the line between male and female has been drawn from the 19th century to the middle of the 20th century. First, I offer an historical account of the ways bodies have been differentiated according to their "sex" in the biomedical sciences. To do this, I present Thomas Laqueur's paradigms of isomorphism and incommensurability to later map the history of the

biomedical sciences onto these two categories, namely in the histories of genetics, embryology and endocrinology. I then discuss how the social sciences have temporalized and conceptualized the changes of categories described in the first part. If the paradigms put forth by Thomas Laqueur captured how the differences between bodies have been thought of, this second part rather presents an analysis of how the *relationship between* biological differences (sex) and social differences (gender) is theorized. This is done with the help of Nicole-Claude Matthieu's periodization of this relationship.

Isomorphism and Incommensurability

Thomas Laqueur argues that there are two paradigms to think of sexual differences. The first, and main one until the 18th century, thinks of these differences in terms of degrees. In this isomorphic paradigm, the differences between male and female bodies are understood as matters of degree, i.e. they are not different kinds of bodies, but more or less perfect versions of the same body. Indeed, the female body is explained by the incomplete development of a single human body of which the male body is the ultimate achievement. A woman is therefore a primitive, even degenerate, man (Gallagher and Laqueur 1986). Since the logic of generation and degeneration stipulates that that which is bigger is more developed[1], the vagina, uterus and ovaries are, in this paradigm, merely an incomplete penis, scrotum, or testes. The female organs are thus understood as dormant, waiting to be activated to fully develop.

The second paradigm, that of incommensurability, rather thinks of these differences in terms of differences in kind. In other words, these are qualitative differences and not quantitative ones. According to this paradigm, therefore, there are two different types of bodies: that of men and that of women. A woman is the opposite of a man, with organs, functions, and emotions immeasurably different. These "types" are so different, according to this paradigm, that it is futile, almost crazy, to compare them.

Though these paradigms can both be present and mobilized at the same period according to Laqueur, he nonetheless argues that the paradigm of incommensurability is favored from the

[1] Aristotle

19th century on. This is explained, for the most part, by the spread of 18th century humanist ideals, according to the historian. Indeed, the scientific revolution of the 17th century nurtures optimism about the human intellect and its ability to know through reason. This leads to a variety of efforts to explain human and social behavior by natural, i.e. rational, laws. Social contract theories are an example of these attempts to explain humans and societies by natural laws. To give up some right in exchange for security and stability, as the social contract theorists propose, is a rational bargain. Since reason is part of human nature, it is therefore a natural law, as opposed to providential or feudal law.

In theory, this humanistic optimism for the force of reason affirms the equality of all citizens. In practice, however, we know that women and minorities are kept out of this idealized 18th century emancipation, of political and educational dimensions. Appeals to authority or tradition to justify the oppression of minorities, however, are no longer compatible with this rationalist humanist tradition[2]. Thus, the effective tutelage of women and other minority groups has to root itself in some "rational" explanation. Failing that, the rights granted to the majority must, in principle, also be extended to them. Because of this, there must be natural causes that explain the effective tutelage of women and minorities. These natural causes, it is thought, ought to be found in their nature. Hence, there must be something in the biology of women and minorities that justify their subordination. To further explore this line of argument, I turn to the explanation of Anaïs Bohuon.

To explain the shift towards the paradigm of incommensurability in science, and a search for a natural cause to the subordination of women, Bohuon proposes an economic explanation. That is, the economic circumstances of the 18th and 19th century necessitated, according to the sociologist, the separate spheres paradigm, as it is known in women and gender studies. The separate spheres paradigm is to societal roles what Laqueur's incommensurability paradigm is to the body.

[2] Though the "domination" of children does not contradict this humanistic logic since they are not at their full development and therefore still need to be guided. The domination of humans who have not reached their full development is therefore rationally justifiable according to the humanistic logic. They have not yet emancipated themselves from themselves.

In part, these socio-economic conditions have to do with the rise of nationalism in the 18th and 19th centuries (Bohuon 2012). Indeed, the imperialist ambitions of nationalism, particularly in the first half of the 19th century, encourage massive state investment in biomedical research. The success of any good imperial project at the time depends on the strength and quantity of its military workforce. To defend and extend the nation, it's citizens need to be quantitatively and qualitatively strong (Bohuon 2012; Gaudillère 2012, 75).

Another factor that encourages state investment in biomedical science research at the dawn of the 19th century is the reshaping of universities. Indeed, the industrialisation of production in the 19th century is a major vector of economic growth and social change. How it reshapes[3] cities has been abundantly discussed by scholars, but its impact on universities is often overlooked. Indeed, the scholastic university model of the 14th and 16th centuries — focused on the reproduction and transmission of what is already known — is abandoned in favor of the German model, focused instead on the discovery of new knowledge. This new humanist university prioritizes freedom and innovation in research over deference. It also favors seminars and research laboratories as learning and teaching methods. These more horizontal teaching styles and new epistemological values form a *milieu* that favors the proliferation of academic disciplines. The disciplines we will see below (genetics, embryology and endocrinology) emerge out of this context.

Moreover, this restructuring of the city, stimulated by the hunt of thousands for work in the city factories, leads to overpopulation. Indeed, the urban exodus brought about by the industrialization of the late 18th and early 19th century drives an unparalleled demographic concentration in the cities. This has a major impact on sanitation – it aggravates the cities' already existing health problems and generates others. Bodies amassed in precarious and grimy edifices, as well as the general popular poverty, lead to a faster and greater spread of illnesses in the cities.

Second, the reshaping of cities means that neighbors became strangers, something that cultivates, according to Ilana Löwy, a climate of suspicion and encourages the development of

[3] For the choice of grammatical tense, I follow Johannes Fabian's Time and the Other: How Anthropology Makes its Object (1983). He argues that the past tense tends to congeal the object of the study whereas the present allows future changes.

techniques of control. These techniques of control, Löwy writes, generally target people's bodies and particularly their sex and sexuality. This can be seen, she explains, by the stabilisation of the concept of homosexuality by the mid 19th century (Löwy 2003, 84; Foucault 1976; Weeks 1981). The thesis of a deception-anxiety that encourages research into sexuality is also supported by Elizabeth Reis's study of intersex in the United States between 1620 and 1960. She finds that the 1830s and 1840s "saw a particular anxiety about the dangers of deception. At a time of increasing geographic mobility and urbanization and the development of new impersonal commercial networks, new opportunities emerged to remake one's self and perhaps to deceive others" (in Magubane 2014, 773).

Further, since women do not go to war and do not significantly participate in early industrial work, the role they are attributed in these two projects, imperialist ambitions and industrialization, is that of mothers as "producers" of future citizens and workers. Such a socio-economic organization of society therefore has the advantage of keeping the masculine and feminine spheres separate: men work and go to the front; women produce future workers and soldiers. This is exacerbated during the 20th century, especially in the period between the two wars. Indeed, historian Jean-Paul Gaudillère notes that in Germany,

> By systematizing medical examinations prior to marriage, the racial laws of the mid-1930s created a space in which the control of reproductive capabilities could take on a mass character (Gaudillère 2012, 75).[4]

Thus, in addition to being encouraged to invest in research, the state is encouraged to develop pro-natalist programs (Oudshoorn [1994] 2003; Gaudillère 2012, 69; Hausman 1995). These policies and orientations are consistent with the management of women's bodies by the medical profession. That is to say, the construction of the female body as object of study by the medical sciences in the 19thcentury passes through the lens of their reproductive and maternal function. The research on estrogen in this period is mainly conducted on account of women infertility (Gaudillère 2012, 70).

[4] « En systématisant les examens médicaux préalables au mariage, les lois raciales du milieu des années trente créèrent un espace dans lequel le contrôle des capacités reproductives pouvait prendre un caractère de masse » My translation.

Another factor intensifies and orients the research and production of hormones in the 1920s. As we will see in the next section, the research into hormones in the early 20th century show that the line between the sexes is blurred. What's more, the socio-political context of the Weimar Republic opens up fears about a loss of heterosexuality and a feminization of men. This is explained by Gaudillère in the following:

> Concerns arose both from the defeat of 1918 and the rise, during the Weimar Republic, of new sexual and social practices that called into question the traditional attributes of masculinity and femininity. Through a discourse on the loss of power of men, their excessive feminization and its possible biological causes ... the spectrum of biological degeneration long associated with eugenic discourse was revived (Gaudillère 2012, 74).[5]

That is, once again, the threat to heterosexuality reinforces the need to gate keep the gender boundaries.

We have identified many factors explaining how research on women's bodies evolved, as an attempt to explain how these researches have resulted in giving natural explanation for women's inferiority. These factors are, the investment into biomedical research, which is, in turn, fueled by nationalism; the reshaping of universities, and the densification of cities. The latter also leads to some anxiety regarding unknown sexualities, which leads to a stabilisation of the notion of homosexuality when both doctors and psychologists focus on the matter. This, in turn, encourages research into hormones. On the other hand, nationalism, and the ensuing wars fueled by it, participate in the vision of women as producers of the future citizens and hence to natalist policies. This further encourages research into fertility and hence hormones. In turn, the research into hormones, which initially shows the blurred lines between the sexes, reinforces the need to keep sealed the boundaries between men and women. We have so far mostly considered socio-political factors that explained the particular shape that took the study of women's bodies. We shall now turn to the specific history of the biomedical sciences regarding what and how they

[5] « Les inquiétudes, suscitées aussi bien par la défaite de 1918 que par l'essor, durant la République de Weimar, de nouvelles pratiques sexuelles et sociales qui remettaient en cause les attributs traditionnels de la masculinité et de la féminité, s'exprimèrent notamment par le biais d'un discours sur la perte de puissance des hommes, sur leur féminisation excessive et sur ses possibles causes biologiques… le spectre de la dégénérescence biologique associé de longue date au discours eugéniste fut de la sorte rajeuni ». My translation.

differentiated between bodies, i.e. what made it that, in these sciences, certain bodies were labelled as "women" and others "men".

Incommensurability in the Biomedical Sciences: Dimorphism

The question of why or how certain bodies are labelled as women and others men rests on considerations that go as far back, in the 19th century, to what causes certain body to express female or male characteristics. In their classic work of 1889, Geddes and Thomson estimated that by 1800, more than five hundred theories on the subject of *sexual determination* are in circulation (Maienschein 1984, 458). Most of those are what historian of science Jane Maienschein calls "externalist" theories. That is, these theories explain the birth of a boy or a girl by factors "external" to the organism (e.g. a cold dark moon, a down economy, what the mother ate, her levels of stress, or even her body temperature at the time of conception) (Webster 2018; Maienschein 1984, 461). These theories receive a lot of attention in the 1880s and early 1890s, but are soon replaced by internalist theories from the 1890s onwards — something that can be explained by the successes of the German experimental embryologists in the late 19th century. Indeed, since Darwin's *On the Origin of Species* explanations about how certain characteristics are passed on from parents to children[6], interest in embryology is effervescent. This being said, how or why an embryo becomes male or female[7] remains a mystery throughout that century.

Hence, the first theories are put forth by experimental embryologists are "internalist" theories, and they dominate the explanation of sex determination until the 1900s, according to

[6] Of course, the full understanding of heredity is not complete without Mendel's work, rediscovered in 1900, but Darwin's book revolutionize how we thought of our bodies (i.e. of it having much more in common with animals than we would have otherwise liked to admit).

[7] Between the time Mendel wrote his paper and the time it was rediscovered, many things happened in endocrinology and the study of genetics and sex determination. In 1892, Richard von Krafft-Ebing publishes Psychopathis Sexualis, with special reference to Contrary Sexual Instinct: A medico-legal study. In 1903 Otto Weininger publishes Sex and Character, which elaborates a complex biological theory of sex. In 1908, Driesch publishes on the science and philosophy of the organism: Gifford lectures, Aberdeen. The concept of Entelechy, a vitalistic conception, gains traction. In, 1909 Edward Carpenter publishes The Intermediate Sex: A Study of Some Transitional Types in Men and Women. This book presents similar ideas to Weininger "Nature, it might appear, in mixing elements which go to compose each individual, does not always keep her two groups of ingredients --which represent the sexes --properly apart…wisely, we must think -- for if a severe distinction of elements were always maintained the two sexes would soon drift into far latitudes and absolutely cease to understand each other".

Maienschein. After those, from 1905 to 1915, hereditarian theories, she notes, with research efforts focusing on chromosomes, replace those of the experimental embryologists (Maienschein 1984, 460). In what follows, externalist explanations are put aside to focus on these two internalist approaches[8].

Experimental Embryologists and Endocrinology

With Darwin's 1859 book and the rediscovery of Mendel's work in 1900, the "discovery"[9] of the human ovary in 1832 is central to the start of embryology (Brandt et al. 2010). Indeed, it will take some sixty years before heteroclite embryologists research from the mid 19th century coalesces into some coordinated research projects. This seemingly "long" trajectory can be in part explained by the fact that the rotary steam powered printing press only appeared in 1843. This invention creates what historian Peter Burke calls the second knowledge crisis (out of three). That is, like the other knowledge crisis, it creates an unparalleled influx of information (Briggs and Burke 2010). This knowledge crisis, however, also points to the fact that scientific journals and discoveries, from that time on, are much more effectively shared. This revolution, therefore, eases the emergence of modern experimental embryology by allowing international research to be shared faster.

At this time, experimental embryologists, and indeed most 19th century doctors, believed the 'real' biological sex of an individual was explained by the gonads (Löwy 2003, 84; Magubane 2014, 764). Experimental embryologists contend, therefore, that "sex is determined in the egg and manifests itself in morphological and physiological differences within the cytoplasm or nucleus" (Maienschein 1984, 460). These gonads would, according to this theory, influence the

[8] Though I keep Maeinschein's typology of sex determination explanations, I do not stick to her dates. Since her 1984 article, more research on the topic has come out which blur the lines between the experimental embryologist and hereditarian theories.

[9] I won't put discovery in quotation marks every time, but that word always struck me as bizarre to describe the fact that "someone found something that is supposed to always have been there". "Stubble upon" might be closer to what I imagine, though hardly acceptable in this academic context. In my opinion, one can really be said to discover something novel, for example, the result of the interaction of X and Y element. But finding something that no one had found before because of a lack of general interest can hardly be called a discovery. I didn't "discover" my husband's watch in the sofa – I found it. If I fail to find said watch, it might be found bn archeologist in 1000 years, but, again, not discovered. I think the term particularly started to irk me after I read that some 19th century Dr. "discovered" the clitoris. Respectfully, you did not discover anything, sir.

levels of hormones in the body. By in the second half of the 20th century, confusion in sexual phenotype is explained by hormonal cause, by a "supposed perturbation of the internal secretions" (Löwy 2003, 85). That is, the expression of secondary sexual characteristics is linked to hormones. Hence, the history of embryology, with regards to sex determination, soon becomes the history of endocrinology, to which we now turn.

Viennese endocrinologist Eugene Steinach studies the effects of sex hormones on guinea pigs starting in 1904 and publishes, some years later, a paper marking the beginning of "modern experiments on the role of hormones in sexual differentiation"(Fausto-Sterling 2000, 158). His research takes an important turn when he meets German philanthropist Magnus Hirschfeld. The latter theorised, inspired by many similar theories[10] of the time, that homosexuality was a sort of third sex – a hermaphrodism — which, because it was natural, should not be persecuted (Löwy 2003, 84). The conclusion of the collaboration between these two thinkers was that those with "periodic attacks of the homosexual drive," had gonads that alternated between the production of male and female hormones. In contrast, "constant homosexuals" developed opposite sexual organs when, at puberty, their male-hormone-producing tissue degenerated (Fausto-Sterling 2000, 163). The combination of Hirschfeld's theoretical proposition and Steinach's research on the antagonistic nature of gonadic hormones and their impact on secondary sexual characteristics and sexual orientation culminated in experimentations on humans[11].

In 1912, ovarian hormones are extracted using lipid solvents by Henry Iscovesco, and, by 1929, biochemist Adolf Butenandt isolates oestrone — the hormone which determines sexual development in females — in pure crystalline form (Gruhn and Kazer 1989, 57). Within a few years, he had also isolated androsterone (1931), a male sexual hormone, and progesterone (1934) (Macrakis 1989, 190). And so, from the 20's to WWII, these composites were the cause of a period of great uncertainty during which the glandular theory of sexes and the fundamental duality of

[10] Cf Weiniger, Sex and Character (1903); E. Carpenter The Intermediate Sex (1909); Karl Heinrich Ulrichs, *Forschungen über das Rätsel der mannmännlichen Liebe*

[11] With the help of surgeon R. Lichtenstern, Steichach removed one testicle from each of seven homosexual men, implanting in its place testicles from heterosexuals. These experiments would have taken place between 1913 and 1923. It is also reported by Davidsen- Nielsen that it was in fact Danish Dr. Knud Sand who first performed human teste transplant in 1923. In any case, what is clear is that the 1910-1920 decade was a prolific one for endocrinology.

the sexes was questioned by biochemistry (Gaudillère 2012, 64). Indeed, "in Germany, steroids remained accessible preparations without the prescriptions of specialists until the Second World War ... [available] over-the-counter at the pharmacists" (Gaudillère 2012, 70). Because of this, Gaudillère argues that the molecularization of gender in medicine comes from top-down practices (through doctors and researchers) as well as down-up ones (through consumers use of steroids) (Gaudillère 2012, 70).

Genetics or the Hereditary Approaches

The second type of internalist theories, according to Maienschein, are the hereditary approaches, known today as genetics. The word "genetics"[12] is coined in 1905 by English scientist William Bateson to describe the emerging field of study of which he is a major contributing part (Mukherjee 2016, 48). After the rediscovery of Mendel's 1865 paper in 1900, Bateson, also known as "Mendel's bulldog", travels to Germany, France, and the US, to spread Mendel's discovery of a "discrete unit of hereditary information" that could not be "diluted" (Mukherjee 2016, 46; Maienschein 1984, 476). This second internalist group, before being known as geneticists are cytologists, Bateson's original field of study. They contend that sex determination is the result of "various inherited "determinants […] which can be found the nucleus" (Maienschein 1984, 460). Because of this hypothesis, they rely on traditional microscopic cytological examination of the nucleus and chromosomes for their research. Genetics as a field explodes in the 1920s, and, by 1923, the Y chromosome is discovered by American Theophilus Painter, though he proposes a human diploid chromosome number of 48 instead of 46 (Harper 2008, 146; Webster 2018). This will be corrected in 1956 by Tijo and Levan (Harper 2008, 496).

It will take 26 years after Painter's discovery for Canadian scientist Murray Barr to identify the Barr corpuscle in 1949 (Harper 2008, 155). The Barr body or corpuscle represents the inactive X chromosome in a cell. The DNA of this second X chromosome is highly compacted, so that all gene expression has been turned off. With the use of a stain which binds to DNA, that highly compacted X chromosome, or the Barr body, can be seen using a microscope.

[12] From the Greek "genos", meaning to give birth.

It will take 10 more years for Jacobs and Strong to explain the central role of the Y chromosome in sex determination in 1959 (Harper 2008, 158). In that same period, the modern understanding and distinction between primary sex differentiation and secondary sex differentiation is already being tackled. That is, by then, we know that chromosomal sex is responsible for the primary sex characteristics, i.e. the formation of testis or ovaries. It is already suspected, however, that only part of the Y chromosome (a gene), and not the chromosome as a whole, is responsible, for the development of testis, what scientists call the Testis Determination Factor (TDF). As for secondary sex differentiation, i.e., the development of the internal and external genital organs, scientists know, thanks to Alfred Jost's research, that those are triggered by hormones, and that the chromosomes, or the TDF, have little to do with secondary sex differentiation (Kraus 2000, 193).

At this point, we should note that the notion of TDF introduces a fourth way of determining sex (after gonadic, hormonal and chromosomic) i.e. the genic sex. At first, from 1975 to 1984, the TDF is thought to be located on the antigen H-Y from (Kraus 2000, 197). But after a study by Verngnaud and al. confirms the presence of Y specific ADN sequence on the X chromosomes in 1986, the Y chromosome is divided into seven regions and the TDF locus is located in the interval 1 of the short arm of Y. This region is then called the Sex determining Region Y (SRY) gene in 1990 by Andrew Sinclair & al. (Kraus 2000, 198). Although the TDF (which became SRY after 1990) is generally found on the Y chromosome, it can be inactive on the Y chromosome or even located on the X chromosome. What is more, the Y chromosome can undergo a deletion (partial or total) and hence no longer carry the TDF, which results in the formation of ovaries (Kraus 2000, 197).

From this we can conclude that, with regards to chromosomal or even genic sex, the terms "male" and "female" do not apply to one object each, but, each applies to several objects. That is, there are several variants to the two standard karyotypes (46 XX and 46 XY) and, what is more, one karyotype can result in a male or female gonadic sex (Kraus 2000, 203). Thinking only of the standard karyotypes, it seems easy enough to classify humans into the male or female category without much ambiguity. This, however, is complicated when we include non-standard karyotypes. Indeed, "female" individuals can have from one to five X and sometimes a Y; "male"

individuals can have from one to two Y and, in certain cases, from one to three X (Kraus 2000, 205). What is more, as we have seen above, a karyotype can be intersex, meaning that a gene normally found on one chromosome is found on another. Therefore, in itself, chromosomic sex does not allow for a simple dimorphic categorisation. Indeed, one karyotype, be it standard or not, can result in a female or male gonadic sex (Kraus 2000, 206).

To conclude, if one could argue that genic sex was able to indicate a qualitative difference between the sexes as opposed to the quantitative types of differences found in hormonal, gonadic and chromosomic sex, SRY, however, does not allow such an absolute demarcation between the sexes. Indeed, as noted above, it can be inactive in certain XX males or present and active in certain XY females. Moreover, testicular tissues can develop without the presence of SRY (Kraus 2000, 208). Hence, though it might be more coherent and in line with their logic if scientists based themselves on the genic sex to categorize individuals into two groups, the classification would nonetheless be arbitrary (Kraus 2000, 208). As Krauss writes: "the fact that bicategorization by sex in researches on the genotype is not based on the genic sex but on the gonadic sex, even if it the latter does not offer any qualitative difference, only confirms the prescriptive and non-descriptive category of sex" (Krauss, 209).

To highlight the complex nature of genic sex determination, biologists Heinz-Jurgen Voß in his 2010 "Making Sex Revisited: Dekonstruktion des Geschlecths aus biologisch-medizinischer Perspektive", presents an enlightening table of the genes involved in sex determination. Below is my translation of a table assembled by. The German version can be found in Annex IV. This table presents 21 genes that are involved in the process of sex determination. Voß remarks, however, that the list is not exhaustive. Hence, the common conception that SRY is like a switch that lights on "male" or "female" in the human development is misguided.

Tableau 1. – Genes Involved in Sex Determination

Gene	Locus	Human protein function (mice in parentheses)	Database entry, cf. OMIM (2008)
Genes expressed in an undifferentiated sexual system			

WT1	11p13	Transcription factors, where more than two dozen different isoforms are known to produce different effects; WT1 may be involved in the regulation of transcription of SRY, AMH and SF1; moreover, it might be important in the formation of kidney function, in the case of myogenesis and possibly in the formation of the retina (Mouse: Wt1 protein may also be involved in the regulation of Wnt4 and Dax1, possibly stabilizing post-translationally SRY mRNA, possibly different effect of isoforms of mouse and human)	607102
SF1	9p33	Nuclear receptor / transcription factor, SF1 is attributed a significance in the formation of the indifferent gonad; besides, it is involved in the regulation of DAX1, CYP11A1, STAR, P450 aromatase, of hydroxylases and gonadropins; in particular it causes the expression of AMH and the formation of testosterone (mouse: initially expressed in all steroid-forming tissues, the expression of Sf1 in XX chromosomes goes back to 12.5 dpc)	184757
LHX9	1q31-q32	Transcription factor, more detailed investigations are needed (mouse: participation in training attributed to testes and ovaries; possibly involved in the regulation of transcription of SF1)	606066
LIM1 (LHX1)	11p13-p12	(Mice: transcription factor, meaningful in the formation of head structures, kidney and genital furrow)	
EMX2	10q26.1	Transcription factor, more detailed investigations are needed (mouse: influences attributed to the formation of the central nervous system and in the development of the genito-urinary tract, mice without functional EMX2 protein did not form gonads or genital tract)	600035
M33	17q25	Transcription factor, more detailed investigations	602770

		are needed (mouse: influence in both testicular as well also in ovarian development, it is therefore assumed that M33 acts upstream of SRY)	
Genes that are described as involved in Testis Determination [TDF]			
GATA4/ggf.mit dem co-Faktor FOG2	8p23.1-p22 (FOG2:8q23)	(In some mouse: transcription factor, expression in the indifferent gonadal, then possibly activation of the transcription of SRY and Amh and thus meaningful in the development of testicles [this possibly together with FOG2], possibly together with FOG2 importance in ovarian development; also important in the development of the heart and in described the formation of ovarian follicles)	600576 (FOG2: 603693)
SRY	Yp11.3	Transcription factor, described as significant in the formation of testicles	480000
SOX9	17q24.3-q25.1	Transcription factor, possibly splicing factor; important in the formation of skeletal and connective tissue and in the formation of testicles, possibly also in melanin production; Studies of humans and mice indicate that SOX9 is sufficient for the development of testes and a male phenotype (this also applies in the absence of functional SRY).	608160
AMH	19p13.3-13.2	Hormone; Meaning is attributed to AMH in the regression of Müller's Ganges; in later Embryonic phases are attributed to AMH significance in the formation of ovarian follicles	600957
SF1	9p33	see above	184757
FGF9	13q11-q12	(Mouse: signaling molecule, important in embryonic development as well as in adult organisms in tissue repair mechanisms; production of testicles)	600921
DMRT1	9p24.3	(Mice and various other types of organisms: Transcription factor; high expression rates of DMRT1 homologues in the individual organism	--

		species are associated with the formation of testes, low expression rates with the formation of ovaries)	
DMRT3	9p24.3	(Mice and various other types of organisms: transcription factor, due to a similar expression pattern as in DMRT1, the presence of DMRT3 in different organisms is also homologous to the above mentioned formation of testes)	602424
SOX8	16p13.3	(Mice: transcription factor, meaning increased SOX9 protein activity, possibly also interacting with SOx8 protein with Amh and Sf1, expressing SOX8 in numerous tissues and organs)	605923
ATRX	Xq13	Helicase, importance in the formation of testicles	300032
DAX1	Xp21.3-p21.2	Nuclear receptors / transcription factors; two different isoforms (DAX1, DAX1A) are described. Importance is assumed in ovarian development, but antagonistic and supportive effects in testicular development and spermatogenesis were also observed (mouse: Again, antagonistic and supportive effect of DAX1 in testicular development observed, however, showed no effect on ovarian production.)	300473
Genes described as involved in Ovarian Determination [ODF]			
DAX1	Xp21.3-p21.2	see above.	300473
WNT4	1p35	Signal molecule; possibly WNT4 protein activates the expression of DAX1; It is assumed that WNT4 for the training of Müller tubes is later involved in the development of ovaries and acts antagonistically to the formation of Leydig cells (mouse: In addition to the possible activation of Dax1, repression of Fgf9 by the WNT4- Protein has been observed.)	603490
GATA4-FOG2	8p23.1-p22 (FOG2:8q23)	See above.	600576 (FOG2: 603693)

FOXL2	3q23	Transcription factor; FOXL2 could be important for complete ovarian development (Mouse: In non-functional Foxl2 gene, no influence on ovarian development could be detected.)	605597
RSPO1	1p34.3	Transcription factor; may develop antagonistic effects on the development of testes in cooperation with other factors (mouse: Rspo1 gene is expressed in early embryonic development in numerous tissues and body regions – not only in the genital furrow.)	609595

(Voß 2010)

In this section, to sum up, we saw that doctors from the 20th century counted six factors which influence phenotypic expression. They can be divided into three categories: 1. *Anatomical Sex* (1.1 external genital appearance, 1.2 internal reproductive organs, 1.3 structure of the gonads; 2. *Hormonal Sex*; and 3. *Chromosomal Sex* (3.1 genic sex, 3.2 nuclear sex) (Gardey 2006, 652; Löwy 2003, 89–96; Moore 1968). Chronologically, after the expression of sexual phenotype through anatomic, and gonadic factors, we saw that a molecularization of sexual phenotype through the hormonal configuration was effected. As Kraus noted, however, "hormonal sex does not express any qualitative jumps but a quantitative one" (Kraus 2000, 202). This molecularization of sex through hormones is followed by a "genetization" of sex. This way of thinking sex differences appears more rigid from the start, imposing a dichotomy, but is later found to be more fluid than this as Kraus and Voß' highlight of the multiple genes involved in sex determination show. This scientific way of thinking about sex, as multifarious as it has been, is not the only way. In the next section, we turn to the way social sciences have thought of sex, more particularly, how they've thought about the relationship between sex and gender.

Bicategorization: The Relationship between Sex and Gender

In *L'anatomie Politique. Catégorisation et idéologies du sexe*, Nicole-Claude Mathieu identifies three conceptualisations of gender and sex within the discipline of sociology. In the first, social sex (gender) is undifferentiated from biological sex (sex). Under this paradigm, the feminine

(gender) is a direct and faithful translation of the (female) biological condition and can only be explained by it. The social and biological sexes are, in this first mode of thinking, the same thing. What is feminine is so because it is done by someone with a female body (Mathieu 1991, 232). Women are, therefore, gentle and conciliatory because their biological nature dictates it. In recent years, this has been explained, for example, in biological terms, by women's lower testosterone (Wrangham and Peterson 1997, 239; Fine 2017). Here, we will refer to this paradigm as the sex equals gender paradigm.

The second paradigm proposes that the social sex symbolizes the biological sex, and will therefore be called the gender symbolizes sex paradigm. That is to say, that the first (gender) refers to the second (sex) and is related to it in some way, without being absolutely determined by it. According to Ilana Löwy, this distinction can be attributed to British sociologist Ann Oakley's "Sex, Gender and Society", which is one of the first books in the social sciences that defines the concept of gender as social sex (Löwy 2003, 97). Oakley defines "sex" as describing the biological differences between males and females, and the "gender" as referring to the social classification and organisation of the "masculine" and the "feminine". Interestingly, equating gender to social sex had already been done by John Money in 1955. Indeed, gender "was introduced for the first time [by John Money] to differentiate a child's biological sex from his sexual identity - that is, the to perceive oneself as a man or a woman, and to adopt a behavior conforming to that identity" (Löwy 2003, 87). In this paradigm, if biology is not the only determinant of (gendered) behavior, it is because there is, according to these theories, a number of codes and norms that are learned by individuals, voluntarily or not (Mathieu 1991, 239). Delphine Gardey notes about those codes that they can have to do with physiological ways to hold oneself, behaviours, social roles and types of jobs chosen. Institutions, for Gardey have a very important role in defining what sorts of roles and behaviour an individual must conform to. Those institutions can be the family and school, which in turn pass on social and cultural values (Gardey 2006, 653). The individual may, according to his or her interests, conform to these codes, or be constrained by her social coordinates, the social arrangement in which s/he finds herself. For example, it will be said here that a person with a "female" biology is gentle and conciliatory because she was raised to be so, or because it is what is expected of her. If she is not, it is less by some biological anomaly than by

difficulty, or refusal, to adapt to the norms. This distinction, however, merits some clarification. Indeed, "for feminist critics of the work of Money and Ehrhardt, their evaluation of CAH girls' behavior shows that these researchers systematically confuse gender identity (feeling like a boy or a girl), sex role (playing with trucks or with dolls, love dresses or pants) and sexual desire (homo-, hetero or bisexual)" (Löwy 2003, 88). From this point of view, being "gentle" is a role and not necessarily related to one's gender identity. This being said, this first attempt at separating gender from sex, though limited, helps better explain the variation of roles and behaviors across the same (social) sex. It should be noted, however, that this paradigm does not call into question the bicategorization of biological or social sex.

This is why the third conceptualization, proposed by feminists of sciences and theorists of queer theory, like Ilana Löwy and Judith Butler, respectively, is important. As we have seen in the section on Dimorphism in the Biomedical Sciences, the technical advances of biomedical sciences have revealed that biological sex is becoming increasingly "difficult to define"[13]. Thus, according to Löwy, it is the advent of social and cultural studies of science in the 1970s that enabled an "opening of the black box of 'scientific knowledge about 'biological sex'" (Löwy 2003, 98) and hence the possibility of this paradigm. This third paradigm, therefore, proposes that the obstinacy to confine the heterogeneity of the biological condition to two categories actually comes from the social system which frames or orients the reading of biological data (Mathieu 1991, 255). This paradigm will be called here the bicategorization of sex by gender. It proposes that the network of norms and codes that form genders govern bodies and pushes them to conform to one of the two groups for the purpose of simplifying social interactions. Because of this impetuous towards simplification, the social model of bicategorization is preferred to more complex models where a heterogeneity of sex would be accepted, for example. As a result, the data invalidating the dimorphic model are either ignored or reinterpreted in favor of the coherence of the dimorphic system.

To take other examples of flesh and blood feminist that defend this paradigm, here is what Christine Delphy writes in 1991 : "the division of humanity into two clearly defined and exclusive

[13] Though this difficulty to define sex is not limited to a technoscientific society. We find examples of more than two sexes in societies that have not gone through a technoscientific revolution.

groups - and the establishment of the principle of the superiority of humanity - shapes and defines our understanding of the biological difference between men and women" (Delphy 1991). Anne Fausto-Sterling seconds this when she writes "we may use scientific knowledge to help us make the decision, but only our beliefs about gender - not science - can define our sex. Furthermore, our beliefs about gender affect what kinds of knowledge scientists produce about sex in the first place" (Fausto-Sterling 2000, 3). Another illustration of this paradigm can be found in Judith Butler, who writes that she:

> originally took [her] clue on how to read the performativity of gender from Jacques Derrida's reading of Kafka's "Before the Law." There, the one who waits for the law sits before the door of the law, attributes a certain force to the law for which one waits. The anticipation of an authoritative disclosure of meaning is the means by which that authority is attributed and installed: the anticipation conjures its object. I wondered whether we do not labor under a similar expectation concerning gender, that it operates as an interior essence that might be disclosed, an expectation that ends up producing the very phenomenon that it anticipates (Butler [1990] 1999, 15).

Butler further links this way of conceptualizing the relationship between sex and gender to a third parameter: sexuality. She writes, "under conditions of normative heterosexuality, policing gender is sometimes used as a way of securing heterosexuality" (Butler [1990] 1999, 13). This echoes Löwy's explanation presented in the Historical Background above on the reasons behind the research on homosexuality in the 19th century being encouraged as response to a fear of being deceived. Indeed, the deception Löwy alludes to is the one of homosexuals parading as heterosexuals, for example a lesbian woman parading as a man. By amplifying the distinction between the genders, and policing the boundaries between them, therefore, the goal is to protect heterosexuality.

From this point on, Löwy remarks, the fact that "culture names and gives meaning to "the facts of nature" [can be] interpreted in two distinct ways"(Löwy 2003, 98). First, as an example of "how biological differences are used to legitimize discriminatory practices" (Löwy 2003, 98)). That is, biological differences between the sexes, presented as "neutral markers", in fact consolidate and legitimize male domination (Löwy 2003, 99). Anne Fausto Sterling goes in this sense too when she writes: "biology is politics by other means" (Magubane 2014, 764). Second, "culture names and gives meaning to the fact of natures" can be interpreted in the sense of our understanding

of social sexes defining our perceptions on biological sexes. That is, our perception of social and cultural differences between men and women affects, in this paradigm, the perception of the biologically sexed bodies. Löwy will argue that the reverse, however, is also true, i.e. the perception of the sexed bodies modifies or alters the way cultural differences between men and women emerge (Löwy 2003, 99). That is not to say that these categories are not *real* or that, because of their lack of independence from culture, they should collapse into one another. Löwy aptly writes: "neither sex nor gender can exist outside of a society or culture, but the cultures that shape them are not necessarily the same. Today, biological sex is understood in the light of a specific culture: that of contemporary science and biomedicine" (Löwy 2003, 100). This reflection is of utmost importance for this project for it is taken for granted here that the scientific culture which will be analysed in the last chapters is not the same one as the one we have access to today.

In sum, in this section, we have seen that the biomedical sciences have used different categories for the gendered body through time. The history of the biomedical sciences in the 19th and 20th century helps highlight the contingent nature of the categories of sex, gender, male, and female. In the history of biomedical sciences, we came across three categories for discussing phenotypic expression (that can be divided into six sub-categories): anatomical, hormonal, and chromosomal sex. We saw that the development of these fields were largely oriented by research on homosexuality and female reproduction. Further, we have seen that the social sciences have proposed different ways of understanding sexual phenotypes: through the paradigms of isomorphism and incommensurability. Lastly, the social sciences have proposed three ways of conceptualizing the relationship between biological and social sex (gender) : the sex equals gender paradigm, the gender symbolizes sex paradigm and the bicategorization of sex by gender paradigm.

Sensitizing Concepts

Bicategorization of Sex by Gender; Gender Symbolizes Sex; Incommensurability; Isomorphism; Secondary Sexual Characteristics; Separate Spheres; Sex Equals Gender; Sexual Dimorphism;

Chapter 2– Herstory of Sport and History of the tests

With the peak of the English enclosure movement in the 18th century, fairs[14], and feast days, where most medieval sporting activities used to take place, are gradually replaced by spectator sports through the 18th century, such that sport as we know it takes its full form in England in the mid-19th century. Besides the enclosures, the industrial revolution also has a major impact on sport and leisure. We know the industrial revolution changed our lives in many ways. It changed life's pace, the space it unfolds in – transforming us into *homo-urbanus* — our relationship to work, to time, to family, etc. What may be less central to discussions on the industrial revolution is its impact on our relationship to leisure and sports. Indeed, where time was once structured around seasons and natural phenomena, industrialization implemented a standardization of time — one aligned, this time, on the rhythm of machines. This meant that work and play ceased to seamlessly fall into one another, but that leisure, like work, became confined to certain times. With this paradigm shift comes the modern 'timed encounter', i.e. a game that begins and ends at a preestablished time (Miragaya 2006, 495). Among the first forms of these modern spectator sports, we count, for example, horse racing, rugby, cricket, and lawn tennis (Rogol and Pieper 2018, 3; Miragaya 2006, 487).

Be it in the medieval fairs or in the mid-19th century spectator sports, however, the fact is that women are mostly sidelined (Cahn 1998; Hargreaves 1994). Though we find records of women athletes before the 19th century[15], their participation in sports is better documented after the 19th century. It remains, however, smaller than the documentation of men's participation at

[14] I have not had the time to research this but it is possible that this decline of the fairs have to do with the enclosure of land (mostly between the 16th and 19th century). It is not hard to imagine how this would have made public gatherings more difficult. There is also the fact that industrialisation made it that major merchants could replace small producers form the fairs.

[15] Miragaya notes that the fifteenth century 'royal' tennis player Margot de Hainult, It is also known that Queen Anne loved to hunt and horse race (Miragaya 2006, 488,508). The 18th century holds records of the first female boxer, Elizabeth Wilkinson, who enters the ring in 1722; of women's cricket games (1745) of the first women to drive a hot air balloon (Elizabeth Thible of Lyons, 1792) (Miragaya 2006, 489, 513). All in all, however, these are exceptions more than the rule.

both periods (cf. Cox, Jarvie, and Vamplew 2000; K. E. McCrone 1984; K. McCrone 1988; K. E. McCrone 1991; Guttmann 1991; Pfister 1990; Holt 1990; Miragaya 2006).

Republican ideals of the time confine the role of women to child bearing and rearing as well as general care giving. Indeed, the very idea of the citizen "out in the world" fighting or working for his nation rests on the idea that his wife will be taking care of him and "his" children, freeing up his time so he can dedicate it to his citizen duties. Gilles Boucher de la Richardia captures this when he writes in 1797 that "women's reign was put to a stop on July 14, 1789, because they were brought back to the private domestic space"[16](Viennot 2016, 1).

Besides these republican ideals, the mores vehiculated during the Victorian era in England (1837-1901) go in a similar direction as the Rousseauian ideal of the separate spheres. The expression "household general" is coined by Isabelle Beeton, a Victorian educator, in 1861, before the publication of her 1866 *The Book of Household Management*. This expression captures the idea that the real dominion of women is that of the home (and that only). There, Beeton argues that a woman's role as the household general requires certain "innate female characteristics" such as being good tempered, compassionate, sympathetic, neathanded, quiet, loving, ordered, and tidy (Beeton 1866).

These ideals of the separate spheres also transpire in the biomedical discourse of the 19th century, where the self-appointed task seems to have been to root the idea of the "innate female characteristics" onto biological indicators[17]. During this period, the study of the female body is

[16] « Le règne des femmes a été arrêté le 14 juillet 1789, parce qu'elles ont été ramenées à l'espace privé domestique" my translation.

[17] German anthropologists E. Huschke writes in 1854 that "the Negro brain possesses a spinal cord of the type found in children and women and, beyond this, approaches the type of brain found in higher apes"(Gould [1981] 1996, 129). In 1861, Broca writes that "il est donc permis de supposer que la petitesse relative du cerveau de la femme dépend à la fois de son infériorité physique et de son infériorité intellectuelle » (Broca 1861, 15). Dr. Victor Josée writes in 1895 "Une femme est un être à part, une chose autre, à laquelle la nature a donné d'autres fonctions qu'à l'homme avec lequel elle ne devrait d'ailleurs pas entrer en compétition dans la vie publique qui n'est pas sa place. Une femme n'existe qu'à travers ses ovaires. » in Gallagher et Laqueur 1986. In 1887, the Chairman of the British Medical Association proposes that 'in the interest of social progress, national efficiency and the progressive improvement of the human race, women should be denied education and other activities which would cause constitutional overstrain and inability to produce healthy offspring" (Fossey N. in Cox, Jarvie, and Vamplew 2000). Further, there are also four different theories proposed as to why sports would be detrimental to women. First, the 'vitalistic' theory, proposed that human organs contained only a limited, non-renewable amount of energy and that women had inherently less vital energy than men. Second, there was a widespread notion that the uterus was the

highly influenced by obstetrics and gynecology, for, as we have seen, the nationalist ambitions give women the role of productors of the nation to be and encourage natalist policies (Bohuon 2012; Pfister 1990). The "force" and vitality of the nation is therefore related, in this paradigm, to the vitality of the women who bear its children. Doctors do not agree, however, on what leads to the desired results (of a stronger mother and therefore race) (Hargreaves 1994, 105). Some argue that strong women lead to strong babies and hence encourage women to do sports; others think sport is detrimental to the foetus and therefore dissuade women from practicing them (Fossey, N. "Gender" in Cox, Jarvie, and Vamplew 2000).

This lack of medical consensus, however, does not discourage 19th century women from practicing sports altogether. Crucial to this increased participation is the push for women's education. Indeed, most women's participation in sport develops inside the newly developed English public education system. Historians note that a number of public boarding schools for girls model themselves on the boys' public schools of the time (Miragaya 2006, 515). This can in part be explained by the Victorian era "*mens sana in corpore sano*" idea, a motif that becomes, what's more, central to the Victorian university life (Miragaya 2006, 486).

The push for women's education in the 19th century, on the other hand, can be explained by the centrality of humanist ideals during this period. Because democracy was increasingly regarded as an unavoidable historical process, the education of the people became one of the most central political questions of the time. Education was not simply seen as a right, but as that without which individuals were rightfully excluded from political participation. At the end of the 19th century, the preoccupation with educating the people in order to adjust them to political participation led to a fever of *démopédie*, as Pierre Rosanvallon called it.[18] *Demopedia* is the art of educating or instructing the people (*demos* + *paideia*), an idea that weds the right to vote and

most vulnerable part of the female body and that excessive physical exercise could have inhibiting effect on the development of the pelvis and, as a result, harm child bearing. Third, Dr. Hugo Sellheim's theory claimed that men had strong muscles while women had loose muscles, which were more suitable to stretching than resistance sports (this theory was cited repeatedly up to the 1970s). Fourth, the fear that women could become physically and psychologically more masculine and therefore turned away from heterosexuality might lose their ability to bear children " (Miragaya 2006, 515).

[18] The term is used following Pierre-Joseph Proudhon's sentence: *democracy is demopedia.*

instruction, as though outer (political) emancipation can only be attained through inner (psychological) emancipation. The 19th century focus on education as a pillar of human emancipation, therefore, fragilizes arguments against women attending school and university.

Women in universities hence form sporting teams. The Oxford and Cambridge ladies rowing teams, for example, start competing in 1855. Miragaya notes, however, that in these sports, women "were not judged for speed, but style, which enabled the competitors to exert themselves to a reasonable degree without upsetting the college authorities" (Miragaya 2006, 500). In short, women were encouraged by the Victorian schools to participate in sports, though to a different degree than their male counter parts. This practice of sport, however, is reserved to Victorian women who attend university, a privileged minority. It isn't until 1887, when Madam Bergman-Österberg, founder of Dartford College[19], introduces the Swedish system of physical training into the London elementary girl schools, that working-class girls are encouraged to practice physical activity *en masse* (Miragaya 2006, 504). Hence, we see that bourgeois women who attend university participate in sports as of the early years of the 19th century, whereas working-class English women partake as of the end of the 19th century.

Outside school related sports, by the end of the 19th century, cycling becomes a very popular sport amongst women. Indeed, it becomes the most common physical activity taken up by middle-class girls and women. At first, their women on bikes provoke outrage since it not only increases women's mobility, but is also the cause for the introduction of what is called the "rational dress", i.e. pants (Birley 1993). On the biomedical sciences front, though women on bikes may have caused social outrage, a survey of 48 doctors by the Faculty of Medicine reveals that 39 of them thought that cycling was not harmful to women (Smethurst 2015, 72). Women of the 19th century participate in a number of sports besides cycling and rowing, which include horse racing, cricket, and tennis.

[19] In 25 years, Madam Österberg's opened six such schools across England : Dartford, which Madame Österberg had founded, Anstey and Bedford, founded by her students, with Chelsea, Liverpool, and Dunfermlone, of varied origin. All of these were completely committed to the gospel of Ling.

Herstory of Sport

1900-1939

The Olympic Games are reinstated in their modern form by Baron Pierre de Coubertin in 1896 in Athens. No woman is reported to have participated to these games though a Greek woman, Stamata Revithi, unofficially ran the marathon on March 30th 1896, i.e. one day after the official Olympic event (Miragaya 2006, 150). Women officially competed in the Olympics for the first time at the Paris OG in 1900. Seven participate in the tennis events, three in croquet, two in equestrian events, ten in golf and one in the sailing event[20]. The invitation of athletes at these second Olympics are sent to private clubs. Since these sports clubs are already admitting women — mostly white upper-class — at the time, they pass the invitation along to their members, which explains the participation of women to these Olympics. Tennis, croquet, golf, sailing, and horse riding are deemed appropriate for women at the time in part because they did not require bodily contact. Moreover, these sports can be practiced in full-skirted outfits something that sits well with the tail-end times of the Victorian era mores (Hargreaves 1994; Cahn 1998; Loy, McLachlan, and Booth 2009). At the 1900 OG, sailing is an 'open' sport, which means that men and women compete together. Hélène Barby (U.S.), who sailed with her husband, hence becomes the first woman Olympic medalist (Miragaya 2006, 180).

At the 1904 St-Louis games, women only participate in archery, two events in which eight women nonetheless compete[21] (Miragaya 2006, 153, 548). Miragaya explains the lower participation of women that year by where its location. Indeed, sending athletes oversees to America is a costly operation for an enterprise that is so far mostly European. Women athletes, therefore, are deprioritized. The eight women who compete in St-Louis are, symptomatically, American women.

[20] Mirayaga adds two in ballooning, but it is unclear to me if this was an official Olympic event. In any case, Mirayaga's count, excluding the ballooning events, adds one more Olympian women to the 1900 than official Olympic records, for a total of 23.

[21] Other sources say six participated, but Mirayaga makes a good case for counting eight (Miragaya 2006, 178).

We understand, therefore, that between 1896 and 1904, women's participation in the Olympics is unregulated and varies according to contingent factors, particular to each Olympics' organisational committee. Indeed, in 1900 and 1904, Pierre de Coubertin had more or less lost control on the organisation of the games due to their occurrence in conjunction with universal expositions. We saw that in 1900, invitations to the Olympics had simply been sent to clubs of which, incidentally, women were already members. The 1904 Olympics, on the other hand, happened concomitantly as the US National Championships in archery, a sport in which women were also already participating since 1879 (Baker 1982). Adding to this, by the 1908 London Games, we do not really know whether women are actually allowed to participate to the OG or, if they are, in which sports exactly. In sum, it is as though the presence of women is a second thought, something that is tolerated but not encouraged. Women nevertheless end up competing in swimming and tennis events in 1908.

In favor of this "women's participation as second-thought" interpretation is the fact that, by 1908, no official Olympic document clarifies the status of women's participation. The only rule that spoke of athletes' admission did not even directly mention women. This rule, article 8, reads: "The British Olympic Council reserve to themselves the right to refuse the entry of any competitor without being bound to give reasons for their decision" (Cook 1908). It is nonetheless this rule that Coubertin summons at the 1912 Stockholm Games to reject Hellen Preece's request to compete in the Modern Pentathlon. Later that summer, Coubertin writes a letter in the July 1912 *Revue Olympique* to clarify the matter of women's participation. He writes :

> Would such sports practiced by women constitute a commendable spectacle for the crowds of an Olympiad? We do not think so. But there is another point, of a practical nature. Would we organize separate events for women, or would we accept higgledy-piggledy events without sex distinction, for single and team competitions? This last suggestion would be logical in as much as the dogma of the equality of the sexes tends to spread. Only it supposes mixed clubs, ninety-five times out of a hundred (95%), this will favor men ... Whatever the feminine athletic ambitions, they cannot rise to the point of claiming superiority on men in running, fencing, or riding ... To invoke the principle of theoretical equality of the sexes here could therefore be equated to a meaningless and inconsequential platonic exercise (Coubertin 1912)[22]

[22] « De tels sports pratiqués par des femmes constitueraient-ils donc un spectacle recommandable devant les foules qu'assemblent une Olympiade ? Nous ne pensons pas qu'on puisse le prétendre. Mais il y a un autre motif d'ordre

Coubertin's dislike of women's participation is clear. The letter, however, also raises questions that are still unresolved today. That is, the likelihood of women winning at all if gendered divisions in sports is eliminated. I will come back to this at the end of this chapter. In any case, saying Coubertin did not want women at the Olympics is an understatement. Be that as it may, that same year, two important associations[23] for women' sports are created in France and 57 women compete at the Stockholm OG[24] (Mallon and Widlund 2009).

Two years later, in 1914, Coubertin tries to have women barred from participating in the Olympics again — on three different occasions that same year. Though his propositions are rejected by a "very big majority of the Olympic committee", the French, Japanese, and American delegates actually second Coubertin's motion (Miragaya 2006, 587). The French representative of the time, Franz-Reichel, also proposes to keep women's points out of the national counts, something which is likewise turned down[25] by the IOC (Miragaya 2006, 587). In sum, by 1914, women's participation at the Olympics remains contested.

The first world war puts the question of women's participation at the Olympics on ice[26]. The march of the public's opinion, however, knows no rest. On the contrary, the war changes people's opinion on women's capacities and place in the world. Indeed, though women had been working in factories before the war, the men's deployment forces industries to accept women's participation in non-traditional fields (e.g. munition factories). Besides, the workforce now includes 22% of married women – a great deal more than the prewar 2% (Brandt et al. 2010, 136–278). Though most women lose their jobs when men come back from the front, those two factors

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pratique celui-là. Organiserait-on des épreuves séparées pour les femmes ou bien accepterait-on les engagements pêle-mèle san distinction de sexe, qu'il s'agisse d'un concours individuel ou d'un concurs par équipes ? Ce dernier procédé serait logique puisque le dogme de légalité des sexes tend à se répandre. Seulement il suppose des clubs mixtes, quatre-vingt-quinze fois sur cent (95%), les éliminatoires favoriseront des hommes…Quelles que soient les ambitions athlétiques féminines, elles ne peuvent se hausser à la prétention de l'emporter sur les hommes en courses á pied, en escrime, en équitation... Faire intervenir ici le principe de l'égalité théorique des sexes, se serait donc se livrer à une manifestation platonique dépourvue de sens et de portée »

[23] The French Union for Women gymnastic (union francaise de gymnastique feminine) and Fémina Sport, a Parisian society for women's sport (Miragaya 2006, 557, 565).

[24] 42 of them competing in swimming events.

[25] Interestingly, not by an enormous margin : 66-41.

[26] Though associations keep thriving as is shown by the creation of the Federation des Sociétés Françaises des Sports Féminin in 1916 (Drevon 2005).

change the perception of women's – and mothers'— capacity to work and be physically active. What's more, from the first war on, industrial nations consider the good health of their population an indicator of national power, which makes them more favourable to women's participation in sport (Detellier 2011, 59).

Despite this change in popular and political opinion, Coubertin still tries to have women barred from the Olympics after the war. He fails again; women participate in the 1920 Antwerp Games in swimming and tennis. All and all, however, "women [remain] barred from most events, notably track and field, for the first part of the twentieth century" (Rogol and Pieper 2018, 3). Indeed, track and field is, at the time, the most popular and prestigious of the Olympic sports. It is also considered to be the most masculine. Rogol and Pieper note that "sport organizers believed [it] was too strenuous and grueling for women" (Rogol and Pieper 2018, 3).

As a response to this exclusion of women from track and field disciplines, Alice Millat creates the International Federation of Women's Sports in 1921 and lobbies the IOC to have women track and field events included. Failing this, she organises Women's Olympic games in Paris in 1922, an event that draws around 20 000 spectators. At first, the IOC appears not to be threatened by the Women games. This is indicated by the fact that it still only offers women five events at the 1928 Stockholm Games. Soon, however, the IOC requests that Millat take the Olympic name out of her competitions; she yields, but the Women's Olympics movement stays strong until 1936, year where Millat stops organizing them. Both women athletes and the public continue to show their interest for women's sports. The pressure on the IOC therefore continues to grow which leads to the officialization of women's admission in the OG in 1924 by adding the following to the general rules: "4. Women's participation—Women are admitted in certain competitions in the Olympic Games. The Program will mention the events in which they may participate"(IOC 1924). The next year, Coubertin steps down as President of the IOC and is replaced by Henry de Baillet-Latour.

This appears to shift the IOC's strategy from full confrontation with Millat's Women's Olympic movement to an effort to incorporate and regulate Women's sports. That being said, changes are slow. At the 1928 Amsterdam GO, gymnastics events for women are added, but

tennis is excluded and track and field events are still not open to women. As a result, the British women boycott the 1928 games. A major problem with track and fields is that "women who competed in athletics faced reproach for being masculine" (Cahn 1998). Hence, white, middleclass women avoided the sport because of the stigma, leaving a vacuum that white, working-class women and women of color filled. This provided new opportunities for these women, but it also reaffirmed the idea of track and field as masculine and inappropriate in the context of Western societal norms. It additionally reinforced stereotypes of black women as less feminine than white women (Hargreaves 1994; Cahn 1998; Loy, McLachlan, and Booth 2009). In sum, track and field female athletes experienced the most criticism and scrutiny, and arguably continue to do so (Ritchie 1996, 80–98).

The addition of women track and field events is indeed where we see questions around gender start to emerge. Pressure from the organizers of the Los Angeles Games of 1932 forced the IOC to put the women's track and field competition into the program, although the IOC, claiming medical justifications for its position, rules out the 800-metre run[27]. Despite this limitation of the 800-metre run, women's participation in track and field events in 1932 greatly disturbs the sport community. This is illustrated, amongst other things, by the fact that the first gender test take place in the following Olympics in Berlin in 1936. We will come back to this in the next section on the history of the test.

1945-1969

After the war, from the late 1940s on, and as the cold war starts to take root, "the eastern bloc female athlete's body [emerges] as a visible challenge to the West's normative ideal of what a proper female body should be" (Ritchie 1996, 147). That is, after the war, we can observe a swing back to the traditional heteronormative norms, momentarily left behind during the war.

In 1953, President Brundage attempts to suppress all women's events from the Olympic Games. A majority vote (against his proposition) puts this question to rest once and for all

[27] The women's 800-meter event is not reintroduced until 1960 Rome OG (Miragaya 2006, 569).

(Miragaya 2006, 613). After this, more and more events for women are added to the OG[28]. The general outlook on women's participation seems to shift after 1953.

Visual summary of the introduction of women sports in the IOC

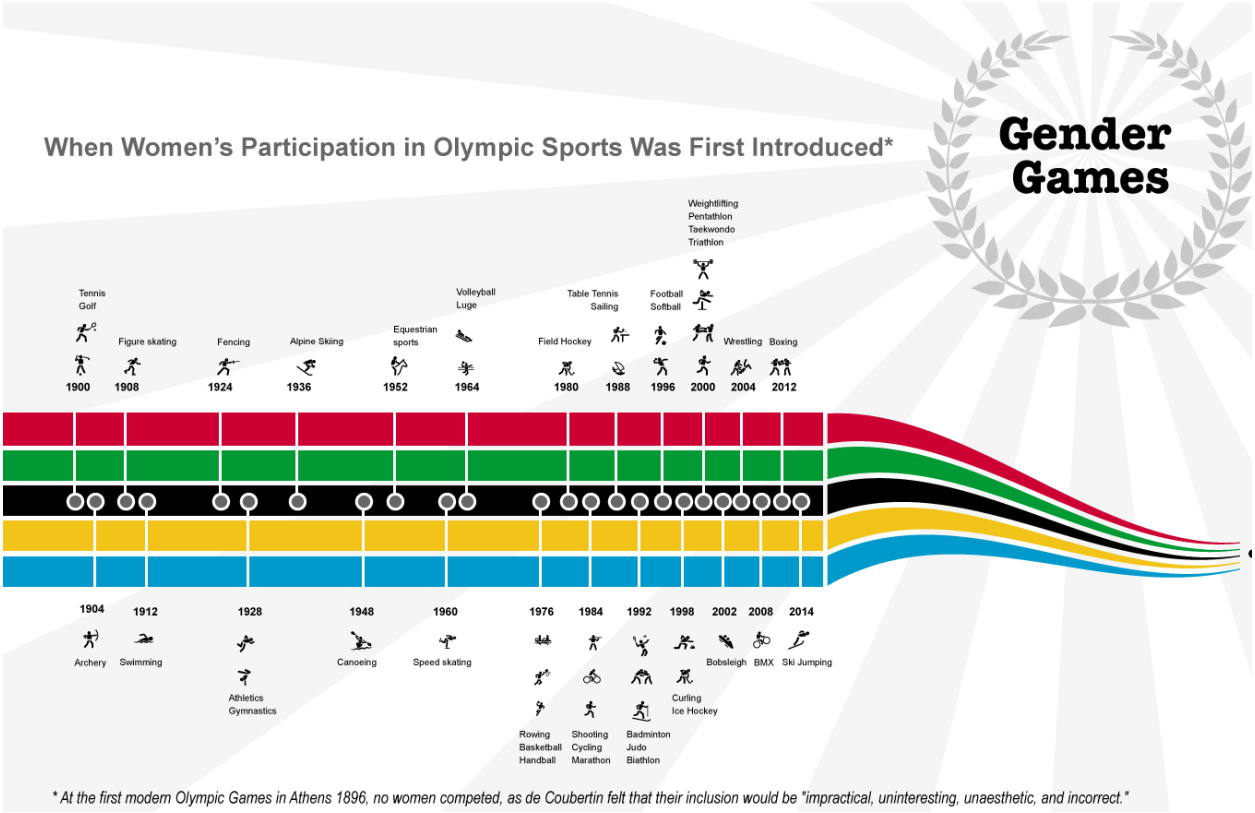

Figure 1. – When Women's Participation in Olympic Sport was First Introduced

Indeed, at the 1959 IOC Session, for example, the Marquess of Exeter "requested the addition of the women's 800m in Athletics programme", stating that the IOC "must go with the time" (Pape 2017, 11). At the 1964 Session in Tokyo, IOC member General Stoytchev describes the under-representation of women athletes as "a shortcoming at the present stage of development of the Olympic Movement" (Pape 2017, 11). In 1967, IOC Vice President Constantin Andrianov makes an even stronger case by noting that although the IOC had declared "the

[28] See Appendix A of *Women in the Olympic and Paralympic Games: An Analysis of Participation, Leadership, and Media Coverage* for a detailed table of the different addition dates of events for men and women (Women's Sports Foundation 2017, 70–76).

principle of equality for men and women in sport, the IOC, at the same time, restrict[ed] women's participation in the Olympic Games" (Pape 2017, 11). Andrianov similarly argued at an Executive Board meeting that same year that in the interests of eliminating "discrimination in every respect," the IOC must "allow more women to take part [in its institutional structure]" (Pape 2017, 11). Further, at the Mexico City session in 1968, when the issue of women in leadership again surfaced, Comte de Beaumont, then President of the French NOC, declared "that there should be at least one female member of the IOC" (Pape 2017, 12).

1970-2019

The 1970s amplifies what starts in 1953. At the Varna Olympic Congress of 1973, for example, a representative of the Polish NOC, Zofia Zukowska, notes that physiologists had found "no health contra-indications in so far as women's sports practice is concerned" (Pape 2017, 12). This being said, Duquin notes that in the late 1970s, however, girls are not given many examples of girls and women being physically active (Duquin 1977). This remains a problem until the 1980s. Indeed, a survey of 1980s textbooks for young children reveals that children are “thirteen times more likely to see a vigorously active man than a vigorously active woman, and three times more likely to see a relatively active man than a relatively active woman” (Young 1980, 154).

Despite this, something starts to shift in terms of representations the 1970s. In the US, the Title IX legislation of 1972 is passed and states that “no person in the United States shall, on the basis of sex, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any education program or activity receiving Federal financial assistance…”(*Title 20 - Education* 2018). From then on, schools and colleges that receive federal funding cannot legally give preference to men. Instead, they have to allocate their resources to men and women in proportion to their interest and enrollment. As a result, a lot more girls start practicing sports in the mid 1970s. In 1972, roughly 300,000 girls participate in high school sports for a staggering 3.2 million girls in 2014" (Women’s Sports Foundation 2017, 38). As for collegiate sports, in 1972, about 16,000 women participate on an average of 2.5 teams per institution. By

2014, those numbers increase to 200,000 on an average of 8.83 women's teams per institution"" (Women's Sports Foundation 2017, 39).

Here is a visual summary

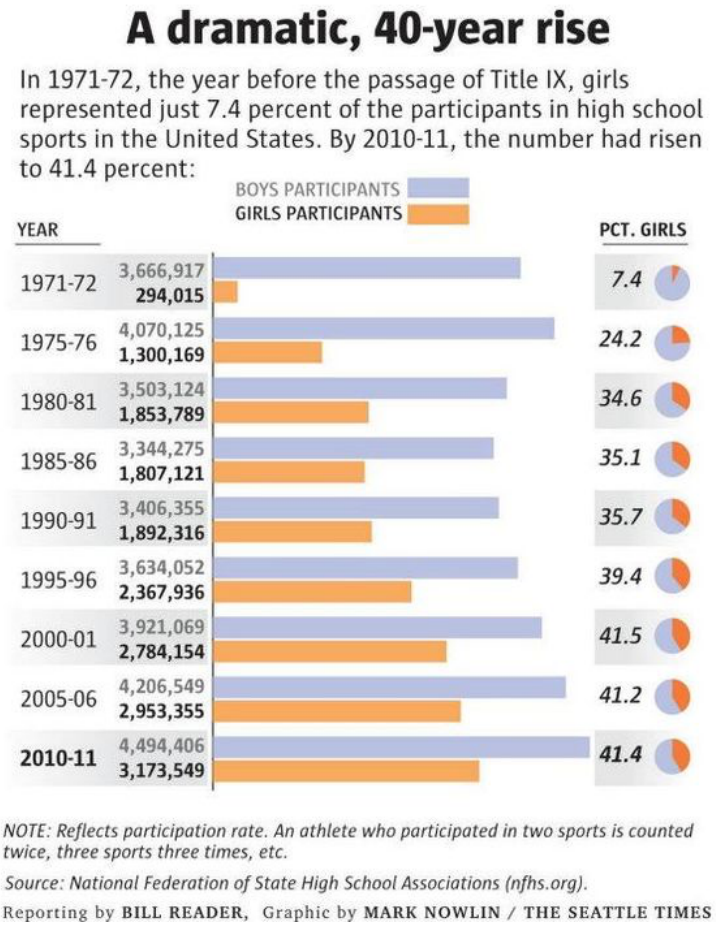

Figure 2. – 40 Years after Title IX : Women's participation in Sport

In sum, the impacts of the changes initiated in the 1970s already start to be felt in the 1980s and 1990s. Scientists of the 1990s even go so far as suggesting that the possibility of a woman becoming the fastest person on earth is just a few generations away. Two studies analyze the shrinking gender gap in running. Their results indicate that if current trends continue, female athletes will outperform their male counterparts. In a 1992 article titled "Will Women Soon Outrun Men?" Brian Whipp and Susan Ward of the UCLA School of Medicine compare the winning times of five Olympic running events that took place between 1900 and 1992, from the 200-meter sprint to the 26-mile marathon. They found that over the shorter distances, women were

improving at a rate double that of men. Over longer distances, this rate is even greater (Bavington 2007, 76). They concluded that women should surpass men by the 2156 Olympic Games, when, for the first time ever, the winning women's 100- meter sprint time of 8.079 seconds would be lower than that of the men's winning time of 8.098 seconds. The current 100 meter sprint for men of 9.58 seconds was set by Jamaica's Usain Bolt in 2009, while the women's world record of 10.49 seconds was set by American Florence Griffith-Joyner in 1988.

At present, "sex-differences in athletic performance in elite-standard track and field competition is of the order of 10-12%" (Ospina Betancurt et al. 2018, 1263). Scientists argue that any possible improvements in performance between the 1983-2016 period "do not affect the calculated ranges of differences in performance between men and women" ((Ospina Betancurt et al. 2018, 1268). In sum, any improvements women may have made between 1983 and 2016 did not affect the performance gap between men and women of 10-12%. In contrast to Whipp and Ward's 1992 study, therefore, women's rate of improvement in relation to that of men's between 1983 and 2016 seems to have stagnated compared to the rate of improvement between 1900 and 1992.

Bavington argues, however, that the reason this gap is not closing and unlikely to close anytime soon is that "male athletes take steroids in much larger amounts than female athletes, but they also have the ability to hide the masculinizing effects of highly androgenic substances that women do not" (Bavington 2007, 76). The continued testing of women's hormonal levels goes in Bavington's sense. Currently, any women above the level stipulated by the IOC is either refused participation or required to lower her testosterone levels. We shall discuss this in more detail in the following section on hormonal testing.

History of the tests

1936-1967, Anatomical tests

The first gender tests performed at Olympic Games took place at the 1936 Berlin OG, when US runner – and hence track and field athlete — Helen Stephens was 'examined' by German officials after having been accused by a polish journalist (Rogol and Pieper 2017, 251; 2018; Wrynn

2004, 214). Remarkably, it was her polish competitor, Stella Walsh[29], star of the 1932 LA OG where she won the 100m run and of the 1936 Berlin OG, who would have failed the gender test had she been so accused (Wrynn 2004, 218)[30]. In any case, track and field athletes cause a stir.

Further evidence of this is that, American Olympic Committee member Avery Brundage, who will later become IOC president, requests, at the 1936 IOC sessions in Berlin, that the IOC consider including gender tests to its events. Though he has other athletes in mind than Walsh or Stephens when he discussed these tests, the two athletes he mentions are also track and field athletes[31]. Following this request from Brundage, the first mention of anything related to gender testing appears in official IOC documents after the Berlin sessions where one can read : "Abnormal Female Athletes: The Committee will for all intents and purposes transmit to the International Sports Federations where female participation is allowed a letter from the American Olympic Committee on this subject"[32] (Miragaya 2006, 598).

In sum, though the first suspicions arise at the 1932 LA games, the genesis of the gender tests really is the 1936 Berlin games. Apart from the suspected athletes already discussed, we should add another to the list: German athlete [Heinrich][33] Ratjen. Though Ratjen competes as a women in the 1936 Olympics, he later lives as a man and returns his medals (Berg 2009). Rogol and Pieper note that "although popular lore suggests the Nazis forced [Ratjen's] faux participation" the public prosecutor explained, after Ratjen had been arrested for "cross dressing" in a train, that "fraud cannot be deemed to have taken place…after all, [Heinreich] had never been told he was a man"(Rogol and Pieper 2018, 5). Hence, at the 1936 Berlin Olympics, five women, all from the track and field discipline, are deemed "suspicious": polish runner Stella Walsh, American runner Helen Stephens, Czech runner and jumper Zdenka Koubkova, German jumper

[29] Stanislawa Walasiewicz
[30] Indeed, after her death in 1980, (she was an innocent bystander at a robbery in Cleveland), an autopsy revealed that Walsh had the sex organs of both a man and a woman (Wrynn 2004, 218).
[31] he mentioned Czechoslovak runner & jumper Zdeňka Koubková and English javelin thrower Mary Edith Louise Weston (Henne 2009, 49).
[32] « Athlètes femmes anormales: Le Comité transmettra à toutes fins utiles aux Fédérations Internationales des sports où la participation féminine est admise une lettre du Comité Olympique Américain à ce sujet ». My translation.
[33] Though Heinrich's « dead name » is known, I chose not to use it.

[Heinreich] Ratjen, and English javelin thrower Mary Weston. Of them, only Stephens will be accosted by the police to undergo gender testing.

After a hiatus in worldwide sport competitions due to the second world war, the IAAF requires, as of 1946, medical documentation from physicians attesting of the sex of all female competitors. The IOC, on the other hand, resumes its activities in preparation for the 1948 London games and follow the IAAF's guidelines regarding the documentation of sex (Heggie 2010). The IAAF is ahead of the IOC on the issues of gender and drug testing, according to Wrynn, because they had "large-scale track meetings on a more regular basis and thus met much more frequently" (Wrynn 2004, 221). The only athlete who "failed" those first iteration of the tests is Foekje Dillema who was "barred from competing by the IAAF for refusing to undergo sex testing or failing it" in 1949 (Rogol and Pieper 2018, 5).

In 1966, the IAAF replaces the medical certificates by "nude parades". These tests consisted in a selection of women athletes parading naked in front of a panel of doctors, first at English Athletic Competition. Then, at the 1966 Budapest European Athletism Championships, this method is extended to all athletes. Mary Peters, an English pentathlon athlete recalls that results had been put up for on a list outside the exam room for everyone to see. The athletes that had "passed" the tests were applauded by the boys on their way out, something she qualified as humiliating (Schultz 2016).

1967-1980 Chromosomal tests

Athletes' disdain of the "nude parades" led the IAAF to introduce a chromatin test for the 1967 European Cup track and field event in Kiev (Rogol and Pieper 2018, 4). According to IOC member and long-time IAAF President, David [Lord Exeter] Burghley, "the test worked so well at their European Championships that track-and-field officials 'managed to keep out six who were hermaphrodites' [and] 'frighten[ed] the doubtful ones away'" (Wrynn 2004, 222). Polish runner Ewa Kłobukowska is the first woman to "fail" the chromatin sex test during the 1967 cup. The IAAF disqualified her and revoked her medals. It was later revealed that Klobukowska had genetic mosaicism, XX/XXY (Rogol and Pieper 2018, 6; Wrynn 2004, 223).

As for the IOC, their first buccal smears are introduced as a trial run on one out of five athletes at the Grenoble 1968 winter games. Officially, "all athletes tested in Grenoble, randomly selected from sports ranging from figure skating, to speed skating to alpine skiing, passed the sex test" (Wrynn 2004, 224). Unofficially, however, Austrian Skier [Erik] Schinegger undergoes the buccal smear test before Grenoble games and fails. Schinegger undertakes a sex reassignment surgery in 1968 (Rogol and Pieper 2018, 6; Wrynn 2004, 223). There is also notes in the meeting minutes that "before the Olympics, we learned from the press that two Soviet and one Bulgarian athletes had preferred not to come to Grenoble" [34] (Medical Commission 1968b Annex II, p.7). The tests in Grenoble are done on a random basis. This not the result of a concerted decision from the Medical Commission, but the result of a sluggish administration. Indeed, the French CNO had only been informed five days before the beginning of the games that "femininity tests" would take place, when the decision had already been made about which tests to conduct back in September 1967[35] (Medical Commission 1968b). That decision of the Commission, however, had to be approved by the Executive Committee before it could be put in place.

How could the CNO put such tests into place in such short notice? They were massively helped by the state. Indeed, though the IOC now generates revenues ("Funding" 2019), mostly through broadcasting rights, and hence funds the games itself, this was not the case at the time of the Grenoble games. Indeed, Avery Brundage then president of the IOC was uninterested in making the IOC financially independent (Cooper-Chen 2005, 231). It is with Juan Antonio Samaranch president in the 80s that the IOC becomes financially independent (Mallon and Buchanan 2007, ci). Without this financial independence, we have to look elsewhere for the speed at which the MC was able to work here. It may be explained by the heavy man-power France provided at the time. Indeed, Charles de Gaulle, then French president, saw the Olympics as a

[34] « Avant les jeux olympiques, nous avons appris par la grande presse que deux athlètes soviétiques et une bulgare avaient préféré ne pas se présenter à Grenoble.»

[35] « Dès que je sus que la recherche de féminité était acceptée par la Commission exécutive du CIO (le 26 janvier) les problèmes pratiques commencèrent à affluer. Il avait été entendu que je devais exécuter ces recherches avant l'entrée en compétition des sportives. Ceci revient à dire qu'en théorie entre le 27 janvier et le 6 février j'aurais dû faire procéder à l'examen des 288 jeunes femmes attendues aux jeux, soit 26 examens journaliers, chiffres qui en lui-même si il est important était encore à la portée de notre laboratoire. Malheureusement lorsqu'il s'est agi de convoyer les jeunes femmes il fallut bien convenir que primo les inscriptions n'étaient pas closes et que secundo les délégations n'étaient pas à Grenoble. »

chance to present Grenoble as a symbol for a modern France. Over 7000 soldiers and employees of the ministries of Youth, Finance, Social Building, Education, Post, Culture and Transport were employed (Organizing Committee 1968, 118). The sum of the investments contributed to 1.1 billion. The government contributed 47.08%, the Isere Department 3.65%, the city of Grenoble 20.07% and the surrounding communities 1.37% (Organizing Committee 1968, 38). Different institutions, such as the train company SNCF; the television broadcaster ORTF; the government housing association and the regional association of hospitals provided the rest of the money.

A member of the Medical Commission, Giuseppe La Cava, reported after the Grenoble games, that he was “opposed to testing only random participants as that practice had 'no logical or biological foundation" (Wrynn 2004, 224). The buccal smear test was then instituted at the 1968 summer Mexico games for all female athlete. After “passing” the test, athletes would receive a “femininity certificate” exempting them from further tests in future events (Bohuon 2012, 65). Though results were confidential at the time, we now know that the 844 women athletes of those games received such a certificate. It is estimated that from 1968 to 1980, "approximately a dozen athletes preferred to withdraw from sport rather than undergo additional testing” (Rogol and Pieper 2018, 4). More importantly, however, Dr. Eduardo Hay, another member of the Medical Commission, remarked in 1988 that “the testing had never uncovered a man falsely posing as a woman” (Rogol and Pieper 2018, 4).

These test were not unanimously praised in the medical community, however. Indeed, physicians and scientists began to question the efficacy and relevance of the test almost immediately. Dr Keith Moore stated in the Journal of the American Medical Association that there were actually nine components of sexual phenotype. Measuring only one of them, such as chromosomal sex - which was the test used by the IOC via buccal smears - was not, he argued, a good indicator of “true sex” (Wrynn 2004, 223). Finnish geneticist Albert de la Chapelle argued that the use of the Barr body test in sport was unethical and discriminated against women with DSD (Differences in Sexual Development) (Rogol and Pieper 2018, 4). De la Chapelle argued in 1986 that “buccal smears were technically unreliable and detected athletes with genetic disorders, such as androgen insensitivity syndrome and gonadal dysgenesis, who were undeniably

female" (Myron Genel and Ljungqvist 2005). In 1980, the IOC switched to polymerase chain reaction test to detect the SRY gene[36]. Based on this test, would not be in the norm.

1990- 2009, Resistance and Hiatus

During the 1983 World Track and Field Championships, twenty-two-year-old Spanish hurdler María José Martínez Patiño received her "femininity certificate". Two years later, during the World University Games, however, she forgets her "fem card" and has to retake the buccal smear test. This time, she "fails" (Rogol and Pieper 2018, 6). After protesting her disqualification, she is reinstated in 1987 (Wrynn 2004, 227). In 1990, a Workshop on Methods of Femininity Verification proposes that laboratory-based sex determination should be discontinued, a recommendation that is accepted shortly thereafter by the IAAF and subsequently by all but 4 of the international athletic federations. By 1992, the IAAF had dropped genetic testing for female athletes, but the IOC persisted, changing instead its' buccal smear test to polymerase chain reaction (PCR) testing (Wrynn 2004, 227).

By the 1996 Olympic Games in Atlanta, "essentially all of the relevant professional societies had endorsed resolutions that called for elimination of gender verification" (M. Genel 2000, 2). Further, "endocrinologists and geneticists pointed out that no one measure could unequivocally identify sex" (Rogol and Pieper 2017, 252). That year, at the World Conference on Women and Health, the IOC passes a resolution to "discontinue the current process of gender verification during the Olympic Games", which are performed for the last time at the Atlanta OG where 3387 athletes are tested.

It takes three more years for the resolution to pass the IOC board. Finally, in June 1999, at a meeting in Seoul, the IOC board to vote to 'provisionally withdrew all sex testing beginning with the 2000 Games in Sydney'. This did not mean, however, that the IOC ended sex testing. Indeed, from then on, if anyone questioned a competitor, tests were conducted by what the IOC called 'appropriate medical personnel' (Wrynn 2004, 227; M. Genel 2000, 2).

[36] Though this test is normally credited to Karry Mullis in 1983, in 1971 Kjel Kleppe wrote a paper describing the method. It is possible that the IOC was a precursor in putting it to use.

2009-2019, Testosterone

This is what happens to Caster Semenya in 2009 after she wins the Athletics World Championships in the 800m run in Berlin. Because of her “masculine” appearance and “low” voice, a mediatic scandal follows her win in which doubts about her “sex” are raised. The IOC, who no longer had explicit procedures as to how to handle these cases since 2000 have her pass a gender test in very chaotic circumstances. This episodes springs the adoption, in 2011, of a new policy by the IAAF. This policy requires that the levels of total testosterone of women athletes be below 10 nM, the supposed lower physiological limit for a young adult male[37], for her to participate in the women category (Rogol and Pieper 2017, 254; Karkazis and Carpenter 2018, 81). Bellow this “male” limit, a male athlete is actually authorized to take testosterone under a TUA (therapeutic use authorization) since it becomes a risk for his health (Hernandez and Vazel 2018). The IOC adopts this IAAF policy under the name of *IOC Regulations on Female Hyperandrogenism* for the 2012 London Olympics (Rogol and Pieper 2018, 2).

With the policy in place for the London OG, four women athletes, all twenty-one years-old or younger from "rural or mountainous regions of developing countries," are identified as having hyperandrogenemia. The IOC bars all four from competing and takes them to France for evaluation and treatment. While in France, the athletes undergo genital reconstructive surgery including clitoral recession and gonadectomy that "ensured" their future participation in sport, in the women’s category. Accodring to Rogol and Pieper, “those opposed to the hyperandrogenemia policy questioned the consent of the athletes and the ethics of performing surgery for eligibility in sport" (Rogol and Pieper 2018, 8).

At the 2014 Commonwealth Games, Dutee Chand is excluded by the IAAF because of her testosterone levels. She protests, taking the case to the Court of Arbitration for Sport (CAS) (Sönksen et al. 2018, 2; Karkazis and Carpenter 2018, 580). On July 27 2015, the IAAF policy is suspended by the CAS (Rogol and Pieper 2017, 254). The CAS gives the IAAF a July 2017 deadline to demonstrate that endogenous testosterone really gives some advantage to athletes. In the

[37] Subsequent research revealed a number of male athletes well below the 10 nmol/L level (Rogol and Pieper 2018, 8).

meantime, the IOC holds a meeting on November 2015 to address both its hyperandrogenism and transgender policies. In regards to hyperandrogenism in female athletes, the IOC encouraged reinstatement of the IAAF policies suspended by the CAS. It also repeated an earlier policy statement that, to "avoid discrimination, if not eligible for female competition the athlete should be eligible to compete in male competition"(Rogol and Pieper 2018, 4). In February 2016, however, athletes learn that the IOC will not apply the policies developed in 2015 at the 2016 Rio Olympics. This is most likely because of the CAS judgement announced after the IOC meeting of November 2015.

As noted by the CAS in the Dutee Chand case, “the Court was not swayed about any data in the literature that "excess" testosterone levels were the sole factor in female athlete performance when a woman with high testosterone levels competed against women with lower levels"(Rogol and Pieper 2017, 254). The CAS panel did accept, however, the IAAF's position that testosterone is a key factor in increased lean body mass (LBM) in males, and that increased LBM confers a competitive advantage in sport. The panel therefore accepted that testosterone is the best indicator of performance differences between men and women. However, the same panel found that there was insufficient evidence about “the degree of quantitative advantage female athletes with hyperandrogenemia enjoy over female athletes without hyperandrogenemia" (Rogol and Pieper 2018, 8). Indeed, because their testosterone production is natural, and not a result of performance-enhancing substances, Rogol and Pieper argue that individuals with genetic conditions that causes their androgen levels to be higher than the norm “should be considered no different from others with genetic conditions that create extremes in height, body proportions, oxygen consumption, muscle bulk, and morphology" (Rogol and Pieper 2017, 255).

In its judgement about the Dutee case, the CAS demanded that the IAAF "establish both the degree of competitive advantage conferred by endogenous testosterone and, if it neared the 12% difference common between male and female athletic performances, [to justify] excluding women with hyperandrogenemia from the women's category of sport” (Dutee Chand 2014). To do so, IAAF doctors Bermon and Garnier analysed the performances of women at the 2011 and 2013 IAAF World Championships. They divided the female competitors into three terciles based

on their free testosterone (fT) concentrations — low, medium, and high — then compared the high fT tertile performances to the low fT tertile ones. The study found statistically significant advantages in five events: 400-meters (2.73%), 400-meters hurdles (2.78%), 800-meters (1.78%), hammer throw (4.53%), and pole vault (2.94%) (Bermon and Garnier 2017).

Rogol and Pieper note, however, that “these differences are well below the overall 10 to 12% difference between men and women in the same events" (Rogol and Pieper 2018, 9). Indeed, the “advantages” range from 1.78% to 4.53%, far from the 10-12% speed gap between men and women. This amounts to saying that the task assigned by the CAS to the IAAF had not been met. Rogol and Pieper further argue that “elite female athletes differ from normal populations with slightly higher circulating concentrations of T and free T (and perhaps other potent androgens) even in the absence of doping" (Rogol and Pieper 2018, 7).

Following this, the IAAF issued a new policy titled "Eligibility Regulations for Female Classification" in April 2018. In this new policy, athletes with a DSD and levels of circulating total testosterone above 5 nmol/L [who are, in addition,] sensitive to the biological effects of testosterone must meet three criteria before being allowed to compete (Rogol and Pieper 2018, 8). First, they have to be recognized by law as either female or intersex (or equivalent). Second, they must reduce their circulating level of testosterone to below 5 nmol/l for at least 6 months. Third, they must maintain this circulating testosterone level below 5 nmol/L (Rogol and Pieper 2018, 9).

The claim that higher natural testosterone provides some women a competitive advantage over other women remains profoundly contested (Bermon and Garnier 2017; Sönksen et al. 2018; Karkazis et al. 2012; Jordan-Young, Sönksen, and Karkazis 2014; Camporesi and Maugeri 2016; Healy et al. 2014). Some authors suggest that testosterone levels are likely to raise with training, and that it is hence the consequence and not the cause of competitive advantage.

In sum, women have been discouraged from practicing sports in as high an intensity as men. In the 19th century, they are allowed or encouraged to practice sport in the spirit of giving birth to healthier babies, preferably male ones, to strengthen the nation. Women officially

compete in the Olympics for the first time at the Paris OG in 1900 but they remain mostly unwelcomed and limited in their participation until the late 1960s. It seems that their inclusion in the track and field events triggered the first suspicions of "fraudulent" participation in 1932, even if the first test end up happening in 1936. The first "tests" is a demand of medical certificate by a doctor. Then, women athletes parade naked in front of a panel of doctors. Both of those tests are what is called "anatomical tests". These are followed by "chromosomic", and "hormonal" tests. To this day, the track and field events still garners the most attention with regards to gender testing with the new policies targeting specifically five events in this discipline (400-meters, 400-meters hurdles, 800-meters, hammer throw, pole vault) in which it is said that statistically significant advantages for women with higher testosterone levels are present.

Sensitizing Concepts

Incorporate and Regulate Women Sports ; Likelihood of Women Winning Without Sexual Divisions ; Western Societal Norms ; Women Olympic Games ; WWI ; AtlantaOG ; DSD ; Male on Steroids ; Medical Criticism of Tests ; Men Women Performance Gap ; Polymerase Chain Reaction Testing ; SRY tests; TitleIX; Women Becoming Fastest Person ; Women Track and Field

Chapter 3 – Epistemological Posture and Theoretical Framework

This master's thesis uses qualitative methods, i.e. methods that prioritize inductive processes as opposed to hypothetico-deductive ones. Inductive approaches stem from different research traditions. Mireille Blais and Stéphane Martineau argue that the tree main traditions that inform the use of inductive methods are grounded theory (see Luckerhoff et al. 2013), phenomenology (see Giorgi 1997) and ethnography (see Latour and Woolgar 1986) (Blais and Martineau 2006, 2). Though all three traditions share the common objectives of problematizing the notion of objectivity and prioritizing an inductive process, each have their particularities. In this chapter, I examine the particularities of those traditions as well as those of feminist epistemologists. My hope is that doing so will make enlighten the reader on the reasons behind the choice of method for this thesis. I start and focus on feminist epistemologies because they capture many of the problems addressed by the other methodologies, and deal with objects that are closer to mine.

Organising life according to differences[38], be they sexual (male/female) or otherwise, is a social praxis. Indeed, feminists have long argued that though it *may* be possible to group humans into two groups based on differences we call sexual[39], the meaning we ascribe to these differences and the impact we allow them to have is profoundly social. That is, what we make of these differences, the psychological and material consequences they have, result from social interactions, alliances — explicit or not — and organisations. Moreover, feminists have argued, these practices enable the emergence of two different cultures (men/women), which, through some combination of feedback loops and confirmation biases, reinforce the significance and importance given to sexual differences. In other words, by rewarding members of an ingroup for a certain set of behaviours and punishing other behaviours, as well as ascribing the converse

[38] Perceived or real.
[39] These so-called sexual differences are an amalgam of physical, anatomical, morphological, and biological traits.

(undesirable) behaviours to the outgroup, each culture participates in exacerbating and amplifying intergroup differences.

This dynamism of differences poses important questions for epistemology. Indeed, many epistemologists argue that knowledge production is a specifically human endeavour, inextricable of its historical, local, and social context (Hacking 1999; Pickering 1992; Pestre 1995; Fleck [1935] 1981; P. Berger and Luckmann [1966] 2006; Feyerabend 1975; Lakatos and Musgrave 1970; Polanyi [1966] 2009; Kuhn [1962] 1996). Indeed, as Löwy writes,

> from the 1970s, some historians and sociologists of science have argued that science is not only a system of statements and a sum of knowledge, but also a practice, or rather a sum of disciplinary practices that are often highly variable, and a collective activity firmly rooted in time and space (Löwy 2003, 99).

As a result, knowledge is, according to them, permeable to cultural or group biases. Combining these two insights — the existence of gendered cultures and the permeability of knowledge to culture — and given that knowledge production has long been in the hands of men, feminist epistemologists began to ask, in the 1980s, whether, and if so, how, male cultural (androcentric) biases impact the production of scientific knowledge. In other words, if we accept that the way gender and gender roles are expressed at any given time is a social praxis, and we further accept that knowledge emerges out of a specific cultural context, then the praxis of gender has to be taken into account as part of the general social context from which this knowledge emerges.

This section is a presentation of my epistemological posture — what I call, with Nicole Ramognino, relational ontology (Ramognino 2013) — through an exploration of the strategies developed by feminist epistemologists in response to the problem of androcentrism in the production of knowledge. Here, I argue that one can identify three categories of strategies in response to androcentric bias. I call these categories *Changing the subject*, *Multiplying the subject*, and *Decentering the Subject*. The better part of this section is devoted to an explanation of these general categories and the particular strategies they harbor. Before going into the typologies, however, let us consider an example[40] of the problem of androcentrism in science as

[40] I am unfortunately limited here to present but one research, but I invite the reader to consult the following for more fascinating and troubling examples: Burt's error presented by S. Gould in *The Mismeasure of Man* (Gould [1981]

identified by feminist epistemologists : a research conducted by British biologist Angus Bateman in 1948, as recounted by Cornelia Fine (Fine 2017).

Bateman, she writes, interested as he was in Darwin's theory of sexual selection, wondered if, and how, reproductive success varied with sexual promiscuity[41]. To test his hypothesis (that it *did* vary), he designed a series of six experiments in which he trapped hundreds of male and female *drosophila melanogaster* (fruit flies) in a jar for a couple of days. These flies, however, were not your 'run-of-the-mill' fruit flies: each one of them carried a mutant gene from a nifty selection ranging from "hairless", to "hairy-wing", to "no-eyes".

A cursory review of some biology notions may be in order here. Genes, which are really sections of DNA, have two alleles. This means that we can carry *up to* two different versions of a gene (but it can also be twice the same!).[42] The typical example to explain the doubling of genes is eye colour. That is, 'within' the section of DNA that codes for eye colour, one could have a side—an allele—that carries the code for "blue eyes" and the other allele that carries the code for "brown eyes". This example might evoke some distant memories about dominant and recessive genes: we are usually taught that the "brown eyes" code is dominant and the "blue eyes" one, recessive. Here, the "brown eyes" gene would be expressed and hence be the person's genotype. What is important to note here, however, is that these terms express a *relationship* between genes, more than some fixed characteristic — in part because gene expression depends on many factors. That is, when one of two different genes carried by an individual is 'expressed', it is called dominant *in relationship to the other*. The recessive gene *in that relationship*, therefore, is expressed only if the other alleles does not carry the gene that is *dominant in that relationship*,

1996);Anne Fausto-Sterling (Fausto-Sterling 2000) on how Thomson and Geddes, with their book *The Evolution of Sex* (1889), shaped the scientific imaginary such that for over a century, ovaries were thought of as passive and sperm as active. For authors proposing that men and women are fundamentally different, and therefore have different capacities, such as success in mathematics or speech, see John Gray's *Men are From Mars, Women Are From Venus* (Gray [1992] 2012) or Deborah Tannen's *You Just Don't Understand: Women and Men in Conversation (Tannen [1990] 2007)*. For an account of the way testosterone has hyped built into the 'male' hormone it is now, see *Testosterone Rex* (Fine 2017). For an example that go beyond the androcentric bias into full manipulation of scientific data to serve political agendas, see *Merchants of Doubts* (Oreskes and Conway 2010).

[41] Unclear to me if he thought of it as a biological trait that could be passed on…or as mere acquired characteristic.

[42] Because I needed it myself, let me give you a brief reminder of how genetic mutation works. Information about how our cells must behave is stored in our DNA. This DNA has a double helix, that is, two lines facing each other, the alleles, related by a perpendicular one. The perpendicular line (or cluster of lines) can be said to be a gene.

i.e. “brown eyes”. In theory, therefore, “blue eyes” could be dominant in relationship to another gene that codes for eye colour (though this has not yet proven to be the case). This relationship between genes, however, is not a mere story domination: the relationship can also be one of codominance, meaning that both characteristics, in this case, would be expressed. That is, if “blue eyes” and “purple eyes” were codominant to each other, one could have a blue and a purple eye. There are some exceptions to this general genotype rule. That is, some people will express more than one genotype due to mosaicism or chimerism. In the former, some cells will lose part of their genetic information and some will be “complete”. For example, a person can have some of his or her cells carry the XX phenotype and others an XXY one. We talk of chimerism when multiple fertilized eggs merge, but humans can carry other humans cells due to bone marrow transplant or being twins and having ingested some of their twins cells in vitro.

In the mutations that interest us, the mutant genes were dominant in relation to the ‘normal’ gene, but codominant in relation to each other. That is, each parent fly in the jars had one ‘normal’ gene and one ‘mutant’ gene, but only the latter was expressed in the parent. As for the baby drosophila melanogaster, they could have any of the following combination of genes: normal-normal; normal-mutant; mutant-normal; mutant-mutant. Bateman’s strategy was therefore a clever way of studying filiation before molecular biology made more precise filiation assessments possible in the 1950s[43]. That is, by using simple maths, he could predict that the mating of these mutant flies should lead to ¼ of the offspring expressing the mutant mother gene, ¼ the mutant father gene, ¼ both mother and father mutant genes (codominance), and the last (lucky!) ¼, no mutation at all. Based on his data, Bateman concluded that male reproductive success increased with promiscuity[44] whereas female reproductive success did not. In other words, having multiple sexual partners increased the number of offspring males had, making of the Don Juan among them some kind of gene spreading heroes. Bateman’s paper, after being forgotten for some time became one of the most cited paper in the field of evolutionary biology. Thanks to Bateman, we have lived with the idea that sexual promiscuity for males is nothing short

[43] It was only in 1944 that Avery, McCarty and MacLeod proved that DNA carried genetic information. Then, Watson and Cricks deciphered the double helix structure of the DNA in 1954.
[44] Having multiple sexual partners.

of a reflex—something 'coded' into their DNA—and justified by the fact that it gives them a reproductive edge.[45]

It took 60 years for evolutionary biologists to re-examine Bateman's data and conclusions. Indeed, it was only in 2007 that biologists Brian Snyder and Patricia Gowaty took an interest that was more than superficial in Bateman's data. They realized that the mutations turned out to affect the viability of the offspring. That is, the baby flies with two mutations were less likely to survive than those with only one mutation and the latter group was in turn less likely to survive than the ones with no mutation at all. This struck them as problematic since Bateman could only count and study the surviving young. Given that the ones that survived were more likely to have only one or no mutation, they thought, Bateman would have only been able to link them to one parent — in the best of cases[46]. Moreover, in the "worst" of cases, i.e. one in which no mutation was expressed, there was actually no way for Bateman to determine filiation – at least not using this technique.

That could have been a marginal problem had Bateman abstained from assigning "double" filiation to the "zero mutation" baby flies. Snyder and Gowaty would find out, however, that instead of leaving filiation undetermined, Bateman actually assigned a parent in most cases and, when he did, tended to favour assigning a male parent. Such that, and this is how Snyder and Gowaty figured out what Bateman had done, in four out of his six series of experiment, his data indicated that males (as a group) had produced more offspring than females (as a group). Given that the reproduction of *drosophila megalonasters*, like that of most species, requires two parents of a different sex, these results are, of course, a logical impossibility[47].

[45] Even if males were a) more promiscuous (which is not proven by this data) b) reproductively advantaged by this promiscuity (again, this data does not show this), the genetic gamble (of promiscuity versus monogamy) is only marginally profitable (if at all) and is therefore a rather weak theory to explain a perceived behaviour. I cannot go into why this genetic gamble is not profitable, but I invite the reader to consult Cordelia Fine's break down in the second chapter of her book Testosterone Rex, titled *100 babies?* for a beautiful rebuttal.

[46] In this case, he would have been able to attribute the sex of the second parent which is less problematic for our purposes, but still worth noting.

[47] For the anecdote: Snyder and Gowaty actually found that, based on Bateman's data, reproductive success actually increased with the number of mates for both females and males, and to a similar degree (Fine 2017).

It is unclear if Bateman did this willingly, but it does not matter: bias can be explicit or implicit, i.e. conscious or not. One could brush this example off as mere human limitation: scientists make mistakes too! Sure. Except one of the most advertised benefits of the scientific method is to be able to rid, or at least contain, these very mistakes and biases, as can be attested by the "father" of the method, Francis Bacon, says about its alleged qualities: "[it is what levels] men's wits, and leav[es] but little to individual excellence, because it performs everything by surest rules and demonstrations" (quoted in Harding 1992, 71).

Changing the Subject

Spontaneous Feminist Empiricists

One could also argue that Bateman's problem could be fixed by using *more* of the scientific method. According to this argument, what happened to Bateman, therefore, is that he did not use the method *appropriately*. Moreover, if it took the scientific community 60 years to catch Bateman's error, it is also because each and every one of them was not scientific *enough*. Cultural bias, i.e. the fact that no one in the scientific community thought to question Bateman's study, is here understood, therefore, as the sum of individual biases. This suggests, then, that we can rid ourselves of these biases in the same way we rid ourselves of individual bias: with *more* of the method. This is in fact the response of the first epistemological strategy we will discuss here: *Spontaneous Feminist Empiricists*.

The reason this strategy is called "Spontaneous *Feminist* Empiricists" and not just "Spontaneous Empiricists" is that when women, and feminists, were first entering the scientific professions *en masse*, in the 1960s and 1970s, their reaction upon noticing these biases, so the story goes, was to double down on the method (Harding 1986). Hence, the attitude towards knowledge was different from spontaneous empiricism in the sense that women's presence in science had an impact on how this spontaneous empiricism was "done" and "understood". For this reason, Feminist Spontaneous Empiricism is placed in the first category of our topology, *Changing the Subject*. It is not so much that these feminists advocated for a change of subject in

science more than the arrival of women in science *did change* the subject in some way and enabled the identification of androcentric bias for the first time[48].

In any case, this response, real or imagined, was not satisfactory for most feminist epistemologists. Indeed, this second group was not convinced that using more of something that had already failed was going to solve the problem. They doubted that the method would be able to operationalize biases, especially those held by the whole community. As Harding puts it, the "methods and norms [of science] are too weak to permit researchers *systematically* to identify and eliminate from the results of research those social values, interests, and agendas that are shared by the entire scientific community or virtually all of it" (Harding 1992, 52). What's more, if Bateman was one of a hand full of examples, one might be justified in brushing it off as an aberration. As mentioned earlier, however, bias in general, and androcentric bias in particular, arises in countless studies, over and over again[49]. Because of this, many feminist epistemologists have continued to propose strategies to operationalize these "collectively held…dominant beliefs of an age" (Harding 1992, 52). One such strategy is *Standpoint Theory*. Some tension exists regarding the genealogy of this strategy, some saying we owe much of it to Black feminists, others that it comes out of feminist materialism. For this reason, I present the strategy with the help of feminist materialist Hilary Rose and Black feminist Patricia Hill Collins[50].

Standpoint Theory

In her 1983 article *Hand, Brain and Heart*, Rose takes the reader through a brief history of the relationship between socialist thinking, and science, and epistemology. She differentiates between two kinds of Marxist relationship to science: the orthodox Marxist analysis and that of the 'new' socialist movements of the 1960s and 1970s. This distinction is made to point out that

[48] It should be noted, however, that this strategy is not, to my knowledge, championed by any flesh and blood feminists, but presented by Sandra Harding in her 1992 article as an "ideal type" of sorts (Harding 1992). From there on, it is taken up by a number of feminist epistemologists in their own discussion of the history of feminist responses.
[49] (Fine 2017; ICI.Radio-Canada.ca n.d.; Perez 2019; Saini 2017; "What Science Has Gotten Wrong by Ignoring Women" 2018; "Combattre le sexisme en science avec des données" n.d.; "Droits Des Femmes En Science et En Technologie : Des Enjeux Éthiques Qui Persistent," n.d.; "The Lancet, 09 February 2019, Volume 393, Issue 10171, Pages 493-610, E6-E28" 2019)
[50] I also chose these texts because both Harding and Longino take up Hilary Rose and Patricia Hill Collins[50] as examples of *Standpoint theorists*.

the orthodox analysis[51] was more optimistic about science — thinking that science might help fix some of the contradictions of capitalism through technological innovations (Rose 1983, 76). In contrast, the later socialist movements, Rose notes, after realizing that "the antihuman technologies that science generated were being used for the profit of some and the distress of many", were more critical of, or at least hesitant towards, science (Rose 1983, 76). Rose presents that latter critical position by going over Alfred Sohn-Rethel's argument.

For Sohn-Rethel, a Marxian critical theorist linked to the Frankfurt School, the antihuman quality of science is explained by the abstract nature of scientific knowledge. That abstract nature, according to him, is due to the social organization of labor in capitalism. That is, labor in capitalism is organized in such a way that intellectual and manual labor are kept separate. On one side, thinking about how the work "should" be done, are the "qualified workers", managers, executives, bureaucrats, etc. On the other side, taking and executing the orders of the first group, are the "unqualified" manual laborers (Rose 1983, 81)[52]. Sohn-Rethel argues that this division leads to the alienation of intellectual labor. In other words, the knowledge they produce is disconnected from and therefore sometimes also a distortion of the world they try to think.

Sohn-Rethel's analysis, though departing from orthodox socialist optimism about science, uses a well-oiled Marxian allegory: the master slave dialectic[53]. For our purposes, it suffices to say about that dialectic that, some have concluded, Hegel ascribes epistemological superiority to the slave. This is because, according to Hegel, the latter is the one who actually has to deal with and transform the world (for the master). Therefore, and to take an example Hegel[54] would probably

[51] Exemplified through John Desmond Bernal.

[52] For a contemporary and fascinating discussion of this division of labor and the qualities ascribed to both groups of workers (e.g. creative, agile, energetic for managerial positions and simple minded, old fashioned, slow for the factory workers) see E. Dunn's Ethnology Privatizing Poland (Dunn 2004).

[53] I initially found the translation of "Herrschaft und Knechtschaft" into "Master/Slave dialectic" problematic. I debated leaving it because, I told myself, it *is* what is most used and therefore recognized in Anglophone philosophical circles. But, still, I struggled. I struggled because I thought Hegel must have been referring to (at least on some level) medieval serfs –for the historical function of the metaphor—and, on an interpersonal level—because the geniality of metaphor is that it represents both a psychological and historical struggle—to any subjugated person in a relationship. So, I thought that, however horrible the experiences of both, to conflate them with slavery was to rob all of them of their meaning and specificity. But I learned, not without joy, that he was a serious reader of the journalistic dossiers on the Haitian revolution of 1792, which leaves some space for the hypothesis that he was indeed thinking of a revolution of slaves (Buck-Morss 2009).

[54] Or any of my philosophy professors…

not approve of, there comes a time where both master and slave realize that the master, though he may own the eggs and may have come up with the idea of making an omelet, doesn't actually know how to break an egg, operate a stove, or what spices go well with eggs, leading to possible catastrophic propositions regarding how the worker should *do* the eggs. Worst, in the absence of the "slave", the "master" risks starvation or food poisoning (Hegel [1807] 1998). If the consequences of this trivial example can already be lethal, when extended to the production process as a whole, they are amplified and, for Sohn-Rethel, explain the dominating and destructive nature of modern science. The dialectic not only serves to show why and how the master slave drama is resolved — by mutual recognition, i.e. (re)valuation of the "slave's" knowledge — but indeed reveals the limitations of nonintegrated or (non)unified knowledge production.

Though Rose agrees with Sohn-Rethel's use of the dialectic as an allegorical instrument to shift epistemic authority, she notes that his project

> attributes the exclusion of women [from science] to ideology and therefore provides no material explanation for [why it so often works to benefit men]. [His critique of science] takes for granted that the domination/subordination, oppressor/oppressed relationship is either irrelevant or explained by the production process" (Rose 1983, 82).

Her premise, therefore, is that the production process alone does not explain the exclusion of women from science. Instead, one must look into the separation between productive and reproductive labor. That is, much like intellectual labor has been separated from manual labor, the division of productive labor from reproductive labor creates a second epistemological alienation. What Rose means by reproductive labor is, on the one hand, the very literal way in which labor gets 'reproduced' — child bearing — but also, on the other hand, what it takes for a human body to replenish itself and come into the office or factory every day to participate in productive labor, i.e. cooking, cleaning, emotional care, etc. Historically, this unpaid reproductive labor has been divided along sexual lines and relegated to women. This alienation of the knowledge produced from reproductive/caring labor, because of the absence of women in knowledge production, is what explains, according to Rose, the dominating tendencies of science on nature, women, and humans generally.

There are two further consequences to productive labor being alienated from reproductive labor. First, women in science have to reconcile their opposing roles as intellectuals and care givers, which makes science a hostile environment for them. Indeed, Rose remarks: "the division of labor for women is different than the mental/manual division of men: even the intellectuals among women ought to be caring" (Rose 1983, 84). Making a similar point, Ruth Wallsgrove writes "a woman, especially if she has any ambition or education, receives two kinds of messages: the kind that tells her what it is to be a successful person; and the kind that tells her what it is to be a 'real' woman" (Rose 1983, 87). Rose notes that this tension experienced by women in science can, however, be a productive tension. Indeed, and this brings us to the second consequence, because women in science have to reconcile these two roles and sets of knowledge — the one valued in their working community and that knowledge which is supposed to make them "good" women—they have what can be described as "fuller" knowledge. Because of this, they can contribute to reintegrate caring labor/knowledge into scientific knowledge production. Rose makes these two points succinctly here:

> The dialectical relationship between both systems of production – the production of things and the production of people — holds the explanation not only of why there are so few women in science, but also, and equally or even more importantly, of why the knowledge produced by science is so abstract and depersonalized (Rose 1983, 84).

This resembles Hill Collins argument. Hill Collins, drawing on Berger and Luckmann (P. Berger and Luckmann [1966] 2006), argues that particular cultures, such as a scientific community, uphold a set of shared dominant ideas, acquired through education and professional training. Further, and like Berger and Luckmann, she thinks it is impossible to "separate the structure and thematic content of thought from the historical and material conditions shaping the lives of its producers" (Hill Collins 1986, 16). Berger and Luckmann call this structure and thematic content "traditional wisdom". Supplementing to their analysis, Hill-Collins argues that this "traditional wisdom" not only reflects the historical and material conditions of the producers, but also reflects the community's practitioners' material positions (Hill Collins 1986, 16). That is, the knowledge that comes out of one's biography — the latter being also thought of as more or less homogeneous with the other community members — is not something that can be crystalized or fixed into a "class" identity, for example. This identity is not immutable but rather

inflected according to the changes of one's positionality and materiality. Hence the difference between Rose and Hill Collins' subject positionality is that whereas Rose seems to suggest that the subject's identity is fixed, e.g. working class women, Collins' subject can be at the intersection of many identities, e.g. working class queer black women, that are subject to change e.g. if this subject acquires wealth, or goes through a gender change.

The important concepts to remember about Hill-Collins are what she calls "insiders" and "outsiders": those who share in X conceptual culture and those who don't, respectively. Insiders, thanks to their shared conceptual culture, have a similar outlook on the world[55]. That outlook is all the more similar or homogeneous if the group members also share the same social class, gender and/or race. Consequently, certain knowledge is taken for granted by the insiders and tints the cognitive schemas of the group. An outsider can acquire an insider group membership, Hill Collins notes, but this requires a long process of socialisation and immersion. Before one is able to "decipher the group's grammar of conduct", the outsider will have to translate the worldview of the other group into her own language until, one-day, her thoughts and actions "automatically" take the shape of this worldview. Only then can the outsider « understand the fine-grained meanings of behavior, feeling and values…and decipher the … nuances of cultural idiom » (Hill Collins 1986, 26). Hence, with time, the outsiders may come to belong to both worlds. From there, an outsider will acquire a new status Hill-Collins calls "outsider-within". This allows them, she argues, to reorient research, through novel interpretation of concepts used by the community, for example, much like the "new" "female" subject of Rose can use her specific positionality and knowledge to influence knowledge production.

That said, Hill Collins goes further than Rose, as alluded to above. Indeed, apart from the fact that Hill-Collins is more interested in redefining the subject of research in social science whereas Rose's analysis is centered around the natural sciences, Hill-Collins' subject is not just women, as is the case with Rose, but Black women in particular. Indeed, Hill-Collins argues that,

[55] For a psychological take on this, see Robber's Cave experiment on ingroup-outgroup bias. Also, Ludwick Fleck's concept of a Denkkollektiv in *The Origin and Development of a Scientific Fact* (1945) and Polanyi's *The Tacit Dimension* (1966). For a more contemporary analysis, see U. Kessels (2009) *How Choosing Science depends on Student's Individual Fit to the « science culture »*.

because of their particular social position, Black women are forced to cultivate a dual understanding, not only of the differences between male and female cultures, but also of that between white and black cultures. If Roses' subject has a dual perspective, Hill-Collins' subject has a *double* dual perspective. This "double dual perspective" is due to the double set of oppressions Black women experience; an insight which Hill-Collins, with other Black feminists, (later) calls[56] the intersectionality of oppression. This intersectionality of oppression, and thus of knowledges, is not reducible to the addition of one set of oppression and knowledge to the other. Rather, this dual experience of oppression creates an altogether new cosmology of oppressions and experiences tied to one's socio-material position which propels "new" knowledge.

Hill-Collins proposes that, in contrast to a view of culture stressing the unique, ahistorical values of a particular group, Black feminist approaches have placed greater emphasis on "the role of historically specific political economies" (Hill Collins 1986, 21). Therefore, the experience of Black women underlines, in her view, the benefits of the tension(s) between outside and inside conceptual worlds that the outsiders-within embodies, which resembles Roses' argument. Going beyond Rose, however, Hill-Collins calls for a knowledge production where "the aim is not to add to existing theories by inserting previously excluded variables, but to develop new theoretical interpretations of the interaction itself" (Hill Collins 1986, 20)[57].

In sum, for the *Standpoint Theorists*, Bateman's methodological failure could be adjusted by changing the epistemic subject, hence their belonging to the *Changing the Subject* category together with the *Spontaneous Feminist Empiricists*. What *Standpoint Theorists* propose for a better, unbiased, science is to change *who* contributes to knowledge production. Rose proposes that women have some advantage over men because they hold two sets of implicit knowledge whereas Hill-Collins argues that Black women, as outsiders from within, have that privileged position from which to produce new theoretical interpretations. *Standpoint Theorists* differ from *Spontaneous Feminist Empiricists* in that the latter ignores cultural biases by collapsing them into

[56] Though the term intersectionality is not coined before Kimberley Crenshaw's 1989 text, I believe we can already see its conceptual workings in this text of Hill-Collins.

[57] This nuance somehow makes me think of Nancy Fraser's distinction, in *From Redistribution to Recognition?* (1998), between affirmative and transformative measures…maybe explore this.

individual biases, and therefore fails to critically engage with them, whereas *Standpoint Theorists* argue that we cannot be rid of cultural biases and that, therefore, we need to rely on what could be called "bicultural" people (with the caveat that who counts as bicultural and how they challenge knowledge production differs for each authors) to get a fuller understanding of biases, or at least get a chance at pointing them out.

Multiplying the Subject

In her 1992 article, Rethinking Standpoint Epistemology What Is "Strong Objectivity"?, Harding presents the following four strategies as feminist responses to androcentric bias: a. *Spontaneous Feminists Empiricists, b. Standpoint Theories, c. Contextual Empiricism*[58]*, d. Strong Objectivity* (Harding 1992). Having seen the first two strategies, we now turn to *Contextual Empiricism*, developed by Helen Longino in Subjects, Power, and Knowledge (1993) and *Strong Objectivity*, proposed by Sandra Harding in Rethinking Standpoint Epistemology What is "Strong Objectivity"?(1992).

In 1993, Longino proposes four main criteria for her *Contextual Empiricism*. First, she argues, the scientific community must be sufficiently diversified to accommodate multiple premises and underlying hypothesis. Therefore, according to Helen Longino, we must pay attention not to reach a consensus to the detriment of critical positions (Longino 1993, 115). To do so, scientific knowledge must be b) separated from the notion of absolute and ideal truth, as well as from that of consensus, if by that we mean it the consensus of the entire scientific community. Therefore, the goals of research must be content to embrace multiple theories that are c) aligned to *local standards of research*, even if those are incompatible. As a result, knowledge has to be d) treated as a practice or as a group of practices. That is, contextual empiricism is committed to an analysis of theory that is model-theoretic. This is an alternative to the view of theories as sets of propositions. Instead, model-theoretic analysis of theories propose that a theory is a specification of a structure; a model that guides our interactions with and interventions in the world. Longino writes, however, that

[58] What Harding calls "philosophical empiricism" to describe Longino's strategy, Longino calls "contextual empiricism". Because of this, I use contextual empiricism.

> given that different subcommunities within the larger scientific community may be interested in different relations or that they may be interested in objects under different descriptions, different models may well be equally adequate and proved knowledge, in the sense of an ability to direct our interactions and interventions, even in the absence of a general consensus (Longino 1993, 116).

I interpret this to means that there is no theory that could fix once and for all a constellation of truth about the world. I'm not sure Longino would approve of this, but the way I make sense of how to evaluate models that may not only be different but in contradiction to one another, is to think of Imre Lakatos' progressive vs degenerative research programs. That is, for Lakatos, there are no *apriori* criteria of evaluation for a research program. Instead, one adopts a sort of consequentialist posture where the virtues of a program (or model, for Longino) are evaluated on the basis of the possibilities they open up for future research. If the research program allows for more (new) data to be integrated without making the model crash, thus allowing further research, then the research program is progressive (Hacking 1999 chp.3). In sum, "good science", for Longino, must be diversified, abandon the idea of consensus, focus on local standards of research, treat research as practices, and engage in continuous reflexivity about these practices and models.

As for Harding, she identifies the main problem of androcentric science with how objectivity is conceptualized. Objectivity, as it traditionally thought of, she argues, is both too weakly and too strongly operationalized. It is too weakly operationalized in the sense that objectivity as we think of it is concerned only with research methods and not with the context of discoveries; and it is too strongly operationalized in the sense that objectivity entails the exclusion of any and all social values, no matter how helpful they may be. Questioning this approach, Harding notes that social values are not all equal. Indeed, she writes, certain values, like those promoting democracy, have in fact "systematically generated less partial and biased beliefs" (Harding 1992, 71). For Harding, therefore, there is both a "better" position from which to do research (certain questions might be better answered by people in a certain position) and a need to position scientific communities into democratic processes. This is what she calls strong reflexivity. It requires both to integrate marginalised perspectives into scientific communities and for scientific communities to be integrated into democratic projects. Hence, Strong Objectivity is about democratizing, or multiplying, the subjects of knowledge.

Like the *Standpoint Theorists*, *Strong Objectivity* and *Contextual Empiricism* see cultural bias as something that is both non-operationalized in science and cannot be ignored. Unlike *Standpoint Theorists*, however, though they might agree that "bicultural" subjects necessarily have some advantage over "monocultural" ones, how they go about integrating those subjects into knowledge production is different from the strategies of Standpoint. Both *Strong Objectivity* and *Contextual Empiricism* aim at diversifying the pool of subjects involved in science in order to combine multiple epistemological viewpoints. This is, in other words, a pluralist approach. Pluralism is different from relativism in that it does not uphold that *just any knowledge goes*. Instead, as seen with Longino, the standards of practices, in a pluralist approach, have to be developed locally, according to what makes the most sense for each discipline and their research objects[59]. For Harding, these standards of research include values (such as democracy).

We now move to a topology of the four strategies presented. I proposed earlier to fit those four strategies into three categories. The first two categories, *Changing the Subject* and *Multiplying the Subject,* are mostly inspired by Longino's categorisation of 1993. That is, I take up Longino's categories, with some changes, as alluded to. I place *Standpoint Theorists* and *Spontaneous Feminist Empiricists* in the first category, *Changing the Subject*. In the second category, *Multiplying the Subject*, I place Harding and her *Strong Objectivity* and Longino's *Contextual Empiricism*. The main difference between Longino's categorisation and mine, is that I propose a third category in which I place *Psychodynamism of Individuation*, instead of putting this strategy in the *Changing the Subject* category as Longino does.

Decentering the Subject

This third category, inspired by Seyla Benhabib, Judith Butler, and Nancy Fraser's discussion in *Feminist Contentions*, I call *Decentering the Subject*[60]. This category is formed by the strategies of *Psychodynamism of Individuation, Postmodernist Accounts of Difference* [61], and *New*

[59] Relate, compare, contrast that to debate between Dewey and Lippman.

[60] I could also have called this category diffractive methods, based on Haraway's proposal of a third way, but I find the proposed name better harmonized to the triad of categories—and the gerund gives it an active aspect I prefer.

[61] This strategy is introduced by Donna Haraway in her 1988 article Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective as a category of strategies. There, Haraway takes up Harding's categories of 1986: *successor science projects* and *postmodernist accounts of difference*. In Situated Knowledges,

Materialism. To sum things up, we now have the following typology: *Changing the Subject* (Spontaneous Feminist Empiricism; Standpoint Theories); *Multiplying the Subject* (Strong Objectivity; Contextual Empiricism); *Decentering the Subject* (Postmodernist Accounts of Difference; Psychodynamism of Individuation; New Materialism). *Postmodernist Accounts of Difference* will be discussed using Donna Haraway and Judith Butler's work. The strategies of *Psychodynamics of Individuation* and the *New Materialists* will be discussed by taking a look at Evelyn Fox Keller and Karen Barad's work.

Postmodernist Accounts of Difference

Haraway proposes to "switch metaphors" in order to avoid the confusion and to hammer in the point that all feminist strategies and responses do indeed aim at finding ways for science to continue, with some modifications (Haraway 1988, 580)[62]. She writes:

> "[O]ur" problem, is how to have simultaneously and account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own "semiotic technologies for making meanings", and a no-nonsense commitment to faithful account of a "real" world, one that can be partially shared… (Haraway 1988, 579).

Haraway, in her 1988 text, is just starting to develop what she calls "embodied objectivity" there (Haraway 1988, 588), but "diffractive method" later (Haraway 1997, 16). I use both as synonyms. Embodied objectivity differs from the traditional conceptions of objectivity insofar as

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Haraway renames these categories *feminist critical empiricists* and *radical constructivism*. I will therefore use *successor science projects* and *feminist critical empiricists* interchangeably and *postmodernist accounts of difference* as synonym of *radical constructivism.*

[62] A bifurcation into a conversation between Harding and Haraway would have been important here for two reasons. First, to show how and for which reasons feminist strategies came to be separated into 'successor science' and 'radical constructivists', and to stress that this failed dichotomy, for Haraway, leads to epistemological stagnation and misrepresentation of feminist efforts. This is important because the accusation of "radical constructivism", which is oft thrown at feminists, is one that implies, often mistakenly, a radical skeptical position *vis à vis* science. In the current socio-political context, and really since the "science wars" debacle of the 1990s, this accusation is a very serious one. That is, "radical constructivists" are presented as undermining science and in this way of siding with science skeptics, something that, in the worst case, can then get amalgamed with flat-earthism or climate-skepticism. I will go deeper into how these two –radical constructivists and science skeptics—are different[62]. For now, however, I want to stress that "radical constructivists" are, at least when we think of the feminists presented here, deeply invested in science. No one would bother trying to "fix" something they do not care about. I hope this will become obvious as I develop the description of the categories of feminist responses to androcentrism further. Second, this detour was necessary to give an account of what is for me the genesis of the third category I'm about to present. Indeed, it is largely due to Haraway and this text that I owe my third category.

the subject/actor is thought of as "material-semiotic" (Haraway 1988, 595). That is, on the one hand, the subject is made up of “stuff” that is both material (e.g., biological) and “supra/infra/proto/crypto material”, for lack of a better word (e.g. language, social interactions). On the other hand, this “material-semiotic-ness” also means that the two—the material and the semiotic — cannot really be differentiated. For example, social interactions, usually thought of as “supra material”, are also material/biological, for Haraway. That is, they have many (if not all) the characteristics we usually ascribe to material things, such as their potency to affect humans and non-human “critters”[63], and potential to be quantified. In the same way, bio-material things are also “supra-material”, if only by virtue of their organization in and by language. Thus, Haraway’s actor is one whose limits “emerge at the intersection of biological research *and* writing” (Haraway 1988, 596 my emphasis).

By defining her actors in this way, Haraway asks us not only to rethink the subject — as one that does not pre-exist, and so is inseparable from, social relations, materiality, or textuality — but also to rethink the object. That is to say that we are asked here to “subjectify” these social interactions, materialities or textualities, traditionally thought of as "objects", or as "passive". In other words, we are asked to think of them as (at least potential) subjects. This does two things. First, it gives them the agency, potency, and qualifications normally attributed to subjects. Second, and perhaps more importantly for Haraway, it demands that we see them as constitutive in their own right of those we usually think of as (autarchic) subjects. That is, in the same way the idea of a subject evokes a series of capacities– agency, freedom, creativity, etc. — Haraway wants textualities and social relations to also be thought of when we think of a subject/actor. Because of this constitutive heterogeneity, Haraway’ actors are therefore "not isomorphic" but rather “agents and territories of stories... heterogeneous multiplicities that are simultaneously salient and unable to be crushed in isomorphic slits or cumulative lists...”. In this sense, I argue that Haraway is asking us to rethink ontology. Her subjects are indeed “partial... never finished... always built and sewn together imperfectly, and thus able to join one another”[64] (Haraway 1988, 584–86).

[63] A term she uses a lot in her new book, Staying with the Trouble (2018).
[64] I would like to eventually relate this conception to Althusser’s Aleatory Materialism.

Not satisfied with a "simple" reconceptualization of ontology, however, or perhaps because it is the necessary consequence of this reconceptualization, Haraway offers an ontology that is inseparable from ethics. Indeed, she writes that “the divided and contradictory self is one that can *query positions* and be a*ccountable*…” (Haraway 1988, 586, my emphasis). It is therefore because embodied objectivity is not an objectivity that unfolds from a “fixed location in a reified body... but on the knots in space, inflections in the orientations” (Haraway 1988, 588) that it encourages, nay, demands, accountability in relation to difference. *This* is the crucial relation between ontology and ethics: this particular type of ontology redefines how ethics is to be thought of. Accountability, in this ontological paradigm, is not a matter of tracing back responsibility to the original misbehavior—let’s say Bateman’s—in order to “punish the mistake” of a “unified”, “coherent”, “free-acting” subject. Rather, this accountability is one which “encourages doubt about one’s own presence as a coherent unit”, and, by extension, encourages a doubt about one’s own (epistemological) authority –and which demands *des comptes* to difference, to where we place the *ligne fatidique* between an ego and everything else. This doubt is loaded with ethical consequences for it allows “the possibility of networks of connections”—by questioning this unified and coherent image of the self—which are “called solidarity in politics and shared conversations in epistemology” (Haraway 1988, 584). In sum, by acknowledging the locality and the semiotic materiality of the subject, Haraway stresses the political and ethical dimensions of knowledge production—but also points to ways of redefining those three projects.

New Materialism

Regarding New Materialism, though many scholars are interested and have written on the topic, I focus here on Karen’s Barad rendering of *New Materialism*. I start by exploring some key concepts presented by Barad, such as: the diffractive method, intra-action, agential cut, phenomena, apparatus. In doing so, we come across problems encountered in the previous conversations, such as the problem of objectivity, a conception of epistemology that is processual, the importance of local-material specificity of knowledge production, and the ethical and political implications of this epistemological and ontological shift.

Barad’s method, which she also calls the *Diffractive Method*, is also a metaphor, inspired by Haraway (and quantum physics). It is used in opposition to representationalist metaphors of

"reflection". "[B]oth are optical phenomena", Haraway writes, "but whereas reflection evokes themes of mirroring and sameness, diffraction is marked by patterns of difference" (in Barad 2007, 29). According to Haraway, "a diffraction pattern does not map where differences appear, but rather maps *where the effects of differences appear*" (Haraway 1992, 300, my emphasis). This method, therefore, helps better attend to the relational nature of difference.

In quantum physics, diffraction is used to describe what happens to waves when they encounter an obstacle. That is, waves have the ability (unlike particles), to go around an obstacle by bending. This bending is different from a mere change of direction in that they continue their forward motion, though differently, and are thus able to fill the space behind said obstacle (or resume that forward direction – unlike particles who are either stopped by an obstacle or diverted). This is easier to imagine if you think of water waves — but light waves also behave this way[65]! As a result of this bend, the obstacle-encountering-waves are no longer "aligned" or "synced" in the way the none-obstacle-encountering-waves were. What is more, their forward-though-changed-motion makes it that the waves encounter each other in a different way after the obstacle: this creates what is known as *interference*. That is, when the bended waves-sets eventually cross each other, a couple of things can happen. Either they meet when both their crests are at their highest; or when they are at their lowest. They can also meet when the crests are at different heights. In the first instances, the waves are then said to be "constructive", i.e. amplify each other (high wave + high wave = amplified/extra-high wave). In the last instance, they are said to be "destructive", i.e. cancel each other out (high wave + low wave = no wave). How the waves amplify or cancel each other out is what creates the *diffraction pattern*, which is what is ultimately "measured". Sometimes, a picture really is worth a thousand words. Allow me to save us some of them:

[65] It is perhaps more difficult to picture light diffracting because when we put an obstacle in front of it, what we can observe is that the light is blocked – it doesn't seem to "go around" to fill up the space behind the obstacle. On the contrary, a shadow is created where the obstacle blocks the light. The fact is, however, that the blurred borders of shadows are the result of diffraction.

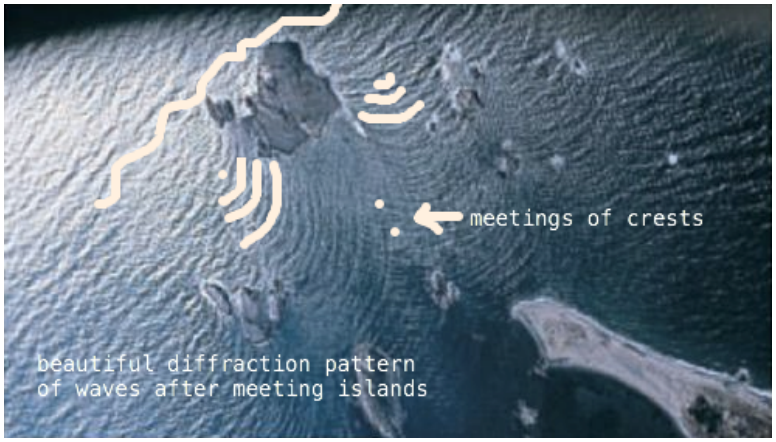

Image XXX

The measurement of the diffraction pattern can be taken anywhere – the distance would only change the recorded intensity, but not the pattern in itself. Imagine, for now, a canvas parallel to pre-diffraction wave at about the distance of the first white dot. The diffraction pattern measured on that canvas would record "big" waves where the crests of the first set of waves meet the crests of the second set of waves (weather the amplification resulted in a positive or negative crest) and "no waves" where the waves cancel each other out. The image bellow expresses what I mean. The multicolor bar is our "canvas" and the wiggly line to its right is a translation into a graph of the recorded pattern. The straight parallel lines are the wave before diffraction and the arched lines are the waves post diffraction. This particular image is about a light diffraction, but this also works for our purposes.

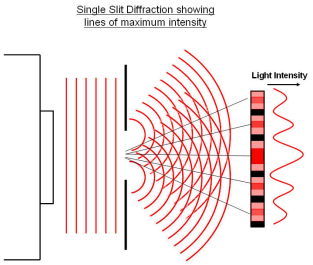

Image XXX

Taking diffraction as a metaphor allows a couple of things. First, it helps shift the focus away from intrinsic characteristics of "things". That is, we can see that what is expressed and hence measured, i.e. that which emerges as "characteristics of the waves", actually emerges from

the meetings of a "prior" set of waves. Because any particular measured "crest" does not actually exist before its' meeting with the other crest, and/or the meeting with the diffracting apparatus -here, the island-, Barad calls this meeting an *intra*-action in contrast with an *inter*-action. The nuance is important for the latter, inter-action, supposes some "agents" formed "before the meeting". This move from inter-action to intra-action therefore requires one to shift "the primary epistemological unit (from "things"[66] to phenomena)", i.e. "from an interaction between preexisting relata to boundary forming intra-action in phenomena" (Barad 2003, 818). Shifting our analysis from "objects" to "phenomena" also underlines the inseparability of "intra-acting" "components" (Barad 2003, 815). This, stressing the inseparability of components and the boundary formation through an interaction, is the ontological revolution.

Second, thinking about diffraction is a helpful metaphor because diffraction, especially in the case of light, is hard to observe without some special apparatus[67]. Indeed, without the use of a certain set of tools, it may have always been assumed that light only behaved in a particle-way. This metaphor hence reveals the way in which humans are both limited in their observations but are also shaping matter to fit into these experiments. There are two things here. First, this emphasizes human's (in)capacity to detect, observe, or fully grasp something without a special

[66] Though Barad seems to keep the distinction between thing and object, she still seems to use "thing" in a substantivized way. "Thingification—the turning of relations into "things," "entities," "relata"—infects much of the way we understand the world and our relationship to it" (Barad 2003, 812). Stressing the distinction between "object" and "thing" might benefit us and better represent the works of phenomenologists who paved the way for the present discussion. Indeed, the old Germanic word 'Thing' means something like 'gathering'. Thing was the name of the tribal democratic institution where people 'came together'. The modern German word 'Ding' comes directly from that. A "thing" is therefore something that 'gathers' meaning, one could say, trough relations. It creates a 'Versammlung' (this word means 'gathering' and 'collection') where the participants create something together. The phenomenologists' "Ding an sich" (thing in itself) is therefore in contrast to mere objects, which are the result of our interaction and *decoupage* of the world. "Ding an sich", on the contrary, cannot fully be grasped or fully unveiled to our consciousness, and so indeed never has a fixed set of 'characteristics'. (I thank Jonas Wiegert for bringing this to my attention).

[67] This, taking in the apparatus as part of the phenomenon, is the strategy chosen by Niels Bohr to resolve the paradox of the wave-particle duality of light. That is, until in the 1920s, it was observed that light not only behaves in a "particle"-way, but that it also sometimes behaves like waves. This created some confusion in physics: is light a particle or a wave?! Bohr's suggestion is that light is both: how it behaves depends on the context in which it is placed, i.e. the apparatus. Hence, his understanding of light is not as some "abstract, independently existing entity but rather the phenomenon of light intra-acting with the apparatus..." which "effect different cuts, that is, draw different distinctions delineating the "measured object" from the "measuring instrument". In other words, they differ in "their local material resolutions of the inherent ontological indeterminacy" (Barad 2003, 815 footnote 21).

set of tools and techniques. Second, it emphasizes the active dynamic role humans take through apparatuses in "(re)configurings the world". This makes apparent the role of "specific agential practices/intra-actions/performances" that come into the constitution of apparatuses (Barad 2003, 816)[68]. Therefore, the metaphor is not only helpful to transition from an ontology of "objects" towards one of "phenomena", underlining the co-constitution of components in that phenomena (what is here called relational ontology), but it is also useful to understand how specific (human) practices shape how differences appear. The method/metaphor helps us understand how our very measuring practices are neither innocent nor inconsequential.

This insight, however, and ironically, makes "idealist" problems reemerge[69]: if everything we do has an impact on the things we measure or want to talk about, how can we ever be sure we are getting the "right" measurements? Further, without an agreed upon "right" measurement, i.e. a shared account of the world, how can we understand one another? I believe part of the answer to this can be found in Evelyn Fox Keller's use of the *Psychodynamism of Individuation*, which we now turn to.

Psychodynamism of Individuation

Evelyn Fox Keller opens her chapter Dynamic Objectivity: Love, Power, and Knowledge by alluding to Bacon's suggestion that "knowledge is power". She points out that, as is illustrated by the biblical use of 'knowing', and contrary to Bacon's infamous proposition that 'knowledge is power', knowledge needs not only be about power but can instead also have to do with connection. Keller argues that this second possibility has been largely ignored(Keller 1995, 116).(Keller 1995, 116) Furthermore, she argues that knowledge's relationship to power has been overblown to such an extent that domination metaphors permeate scientific writing and thinking.(Keller 1995, 123) She stresses that the relationship to nature is all too often expressed

[68] Barad goes further than that, as seen in the previous paragraph. She would even say that the island is an apparatus of diffraction. In this sense, there is no need to ascribe active agency, or agency at all, to humans alone. But let us put that Posthumanist point aside for now.

[69] I think Bergson's response would also be helpful here.

in terms of scientists 'attacking' or 'solving' nature, or of 'conquering' her/it — implying that something about nature will, through the attack, conquest, or discovery of a solution, disappear and make way for the scientist's vision and will.

In Fox Keller's opinion, this conception of knowledge resembles a conception of objectivity in which the knowing subject, in order to be objective, has to be detached from her object of study. This (mis)conceptualization of objectivity is due, she argues, to a specific (mis)conception of autonomy wherein there is a 'tacit implication that autonomy can be bought only at the price of un-relatedness'.(Keller 1985, 72) The shared ideal of un-relatedness in both autonomy and objectivity has persuaded her to explore the interaction between emotional and cognitive experiences and development. Not only does this shared ideal suggest that the two are related, but Keller's hypothesis is that her study could help uncover the idea that they are, in fact, co-constitutive. In order to explore this relation between objectivity and autonomy, Keller turns to the object-relation theory of German psychoanalyst Ernst Schachtel and his take on the 'psychodynamism of individuation'.

In traditional Freudian psychoanalysis, the process of individuation, i.e. ego formation, is thought of as the more or less tragic consequence of a self-awareness, or the delineation between inner and outer stimuli, that develops as a result of unfulfilled needs. Freud writes:

An infant at the breast does not as yet distinguish his ego from the external world as the source of the sensations flowing in upon him. He gradually learns to do so, in response to various promptings. He must be very strongly impressed by the fact that some sources of excitation, which he will later recognize as his own bodily organs, can provide him with sensations at any moment, whereas other sources evade him from time to time — among them what he desires most of all, his mother's breast — and only reappear as a result of his screaming for help.(Freud [1930] 1961, XXI:67)

That is, by realizing that her mother cannot and is not fulling her needs, the child understands that she is not, in fact — and this is contrary to what she may have initially thought/felt — one with her mother or the world. This is a traumatic experience for the child then realizes that she must turn outward to satisfy unmet needs, and this towards an external world over which she has little control. This initiation into self-consciousness is condemned, for Freud, to a separation from the mother/world because it destroys the symbiotic illusion. This makes the child's relationship to the world conflictual, but also de facto oriented towards instrumentalization. Though Freud acknowledges that this feeling of connectedness with the world (or symbiosis) may subside in some adults, something he calls in this context the oceanic feeling, he claims not to recognize it in himself and proceeds to link it to some primitive pre-individuated ego.(Freud [1930] 1961, XXI:64–65) Because of this, the oceanic feeling speaks to, for him, some (regressive) longing to (re)unite with the world/mother.

Schachtel has a different understanding of that dynamic of individuation. For the latter, fulfilling unmet needs is one of two types of interest the child can have for the world. According to Schachtel, the child is first and foremost turned towards the world by a curiosity and pleasure that exceeds biological necessities, i.e. there is an intrinsic and independent joy brought about by merely discovering the world. This joy is explained, in Schachtel's view, by the satisfaction the child experiences when she is connected to others and the world. Further, since Schachtel doesn't think of the dynamic of individuation as traumatic, the kind of uniting effort such as that sought out through Freud's 'oceanic' feeling can be positive and desirable.(Schachtel [1959] 2001, 182) This is the case so long as the transition from the symbiotic moment to the moment of individuation is dynamic. For this dynamism to be possible at all the child must have a secure sense of self, which Schachtel describes as one that can tolerate both difference and continuity between self and world. This in turn enables an attention for the world and its objects that is not

only vested with or contingent upon one's needs and desires.(Keller 1995, 119) Schachtel calls this attention allocentric perception. He calls the instrumentalizing perception which opposes allocentric perception 'autocentric perception'.

Fox Keller uses Schachtel's developments on perception to discuss what I alluded to earlier: a particular conception of autonomy and its relationship with objectivity. Keller terms the traditional conception of autonomy, i.e. one in which one sees oneself as separated from and impermeable to the world, static autonomy. Keller adds, however, that the allocentric perception that I just mentioned allows for another conception of autonomy, which she call dynamic autonomy. This autonomy requires that one trust her capacities and abandon the delusion that she is fully self-sufficient, can act independently of the world and others, or can avoid being acted upon. This will allow 'for that vital element of ambiguity at the interface between subject and object'.(Keller 1985, 84, my emphasis)

The notions of objectivity corresponding to dynamic and static autonomy are dynamic and static objectivity. Dynamic objectivity aims at a form of knowledge that 'preserves the independence and integrity of the world while at the same time recognizing the subjects' relation and dependence on the world'.[70] Keller compares this sort of objectivity to empathy. Like empathy, then, dynamic objectivity is an objectivity that mobilizes shared experiences and emotions between the subject and object of knowledge.[71] In static objectivity, on the contrary, the understanding of the other can only be attained by separating and fracturing the subject from her object of knowledge, which involves dissociating the object from the subject so that the latter can instrumentalize it. This leads to a type of knowledge where difference is thought of in terms of 'frontiers and sharp edges'.(Keller 1985, 121) In this paradigm of objectivity, perception

[70] *Ibid.* p. 117.
[71] *Ibid.* p. 116.

becomes an 'act of aggressive violence in which the perceiver, like Procrustes with his hapless victims, cuts off those aspects of the object which he cannot use for his purposes.'(Keller 1985, 120)

In sum, Fox Keller's vision of the subject is not, strictly speaking, a 'decentring' of the subject in the sense that it makes the subject disappear. In fact, as we have just seen, Fox Keller spends a significant amount of energy describing how a subject is formed, or individuated, and how that impacts her relationship to the world and therefore also to knowledge. My point is, however, that this dynamism of individuation — which at one point accepts a certain degree of separation and at another unites subject and object — forces us to think of the frontiers between subject and object as at least momentarily absent. This 'decentring of the subject' is the key, in my opinion, to answering the criticisms addressed to New Materialism which I have mentioned above[72].

In sum, there are three categories of response to androgenic bias proposed by the feminist epistemologists: *Changing the Subject*, *Multiplying the Subject*, and *Decentering the Subject*. We saw that the *Decentering the Subject* congregated three strategies: *Postmodernist Accounts of Difference*, *New Materialism*, and *Psychodynamism of Individuation*. In concrete terms, for the choice of the research method, Changing the Subject would correspond to having a women do this study. Multiplying the Subject would have required to have my discourse analysis peer reviewed, which was unfortunately not possible. The Decentering the Subject category is closer to Latour's type of ethnography and to Grounded Theory, both of which we now turn to.

Ethnography seeks to observe constructions in their contexts of production. Its task is therefore to "familiarise oneself with a field while remaining independent from it" (Latour and Woolgar 1986, 23). Though there are many different ways of doing ethnography, I turn here to Bruno Latour's version since, as we shall see, and particularly as it is expressed in the Actor Network Theory of Bruno Latour, resemble the epistemological shift operated by the third category presented above. In *Changer de société refaire de la sociologie* (Latour 2007) Latour

[72] This point will be elaborated upon in an upcoming publication, see (Filion-Donato 2020).

contends that sociology inherited the above mentioned modern theoretical framework and, as such, places the actor as the point of departure of action. Sociology thereby distinguishes humans from the rest of the world. Indeed, here, the possibility of "action" is credited to humans, reducing the rest of the world to simple intermediaries, or "simple mute forces". Latour contends instead that the cultural and the natural world are intrinsically connected, especially in a world where technoscience stripped the frontiers between nature and culture and between science and technic. The objects that surround us are, therefore, for Latour, hybrids. He argues, however, that our "modern" categories prevent us from thinking this hybridity. Indeed, Latour criticizes modern rational humanism and claims instead that humans are made of the cultural objects that surround them: cyborg, biotechnology, computers. Humans are composed of cultures, nature, and artifice. For him, therefore, a sociological approach that reduces its analysis to the study of human behaviour can only offer a partial analysis.

Because of this, Latour gives a special status to technical objects as can be seen in *Clef de Berlin* (2006). There, Latour notes, « it is because the social cannot be assembled with social that it needs keys and locks»[73] (Latour 2006, 12). In this text, the key is revealed as a mediator between the locator, the concierge, and the other locators[74]. As such, it is a social actor, an agent, something active in the social relation – something Latour will later call an *actant*. The latter are defined by their capacity to act, to have some weight on things, to have an intensity in how actions unfold. Actants can be human actors, as is traditionally conceived, but they can also be organisations, objects, or metaphysical entities. Using the notion of actant allows sociology to reconcile humans and non-humans as one and the same. Those actant are, Latour argues, all the more to be found in the laboratories. I share this sensibility to objects and techniques and that is why I chose tests as a point of departure to analyse the representations of the body.

Grounded theory is another methodology that is close to what has been done in this research .Grounded theory's main objective is to give as much importance to the empirical data as possible (Glaser and Strauss [1967] 2009). This does not mean, however, to ignore the researcher. On the

[73] « C'est parce que le social ne peut se construire avec du social qu'il lui faut des clefs et des serrures. »
[74] One wherein the concierge can decide if and when, and impose this decision on, the locators have to lock the door on their way in or out of the building, or if they have to leave it unlocked.

contrary, to be really empirical, one must consider the latter as data or at least as something to be considered in the research. This means that the scientist is asked to make a temporary suspension of her reliance on existing theoretical frameworks such that her analysis can emerge from, or be as close as possible to, the data, what will be called an emergent fit. It is a "fit" and not just an emerging analysis because the scientist will go back and forth between this emerging theorisation and her data, ensuring that her theorisation does not depart too much from the data. The scientist confronts her concepts and declarations to the empirical data which allows her to judge of the adequation between her theoretical sketches (Luckerhoff et al. 2013). There is therefore a recursive, almost dialectical process of back and forth between the theoretical and the empirical.

The scientist, according to Luckerhoff & Guillemette, must develop a theoretical sensibility. This sensibility expresses itself in two ways: first, by an attention, a "sensibility", to data and second, by a sensibility to her own theoretical perspective — that with which she perceives and interprets her data. These are called "sensitizing concepts" (Glaser and Strauss [1967] 2009; Blumer [1969] 1986; Strauss 1987; Hoonaard 1996; Glaser 1978; Guillemette 2006, 14). They correspond to the perspectives and source of inspiration that help direct the first steps of the data collection and analysis (Glaser and Strauss [1967] 2009). Sensitizing concepts are not exactly like a theoretical framework, but they do inform the scientists' perspective and allows her to make sense of the data (Tourigny Koné 2014, 89). It is in part because of them that the scientist is "able to transcend first level evidence to discover what seems inaccessible to common sense" (Luckerhoff et al. 2013, 10; Guillemette and Luckerhoff 2009, 13). Sensitizing concepts include the scientists' experiential, theoretical and cultural knowledges (Guillemette and Luckerhoff 2009, 14). These are subject to evolve throughout the project (Tourigny Koné 2014, 90).

According to Guillemette, the scientist develops this theoretical sensibility to her own theoretical perspective by jotting down her apriories on her topic (Luckerhoff et al. 2013). This is not the naïve proposition that one can absolutely suspend their judgement. It is rather an intellectual effort to objectify, i.e. put into the world, what is usually ignored because considered too fickle or multifarious to capture (the subject's thoughts, biases, or mind map) so that these can also be counted as data. The idea of treating those as data will allow to better understand the

relationship between the subject and the object of study, allowing a more thorough analysis of a phenomenon, as defined in the previous chapter in our discussion on Barad. What I have attempted to do in this thesis, to facilitate this for myself and my readers, is to underline what I have identified as my sensitizing concepts in each of the historical and theoretical chapters written so that my perceptive and epistemological frame can be more easily seen, analysed, and possibly challenged.

If the grounded theorists offer the theoretical foundations of this conception of knowledge, it is from Vincent Ross and Paul Sabourin that take the more practical methodological aspects of this relational theory. Vincent Ross, in *La structure idéologique des manuels de pédagogie Québécois,* proposes a method of analysis of ideology on which I will rely here. He offers two levels of analysis: synchronic analysis and diachronic analysis. The first contains five elements: the ideological definitor, the ideological public, the model of action, the ideological premises and the representations of the situation (Ross 1969, 172). The diachronic analysis consists, on the other hand, in comparing these different elements between different socio-historical texts. In the *construction of sociological data* of the next chapter (research strategy), I will define the first three elements of the synchronic analysis in my research object.

According to Berelson, a good category must be homogeneous, exhaustive, exclusive, objective and appropriate (Sabourin 2017, 6). To say of categories that they are explicit and homogeneous is to say that the groupings must be of named surroundings and accommodate objects of size/value/similar type. That is, to use the example of the need to make boxes for a move, it would be ill-advised to make a box "kitchen instruments" and another "furniture". These categories, apart from the fact that the second evokes ridicule in practice, could not, even in theory, store objects of similar size. With respect to the completeness of the categories, it is a matter of avoiding the creation of categories so large that they can contain very different objects, such as a "miscellaneous" category, for example. Exclusivity is similar to completeness insofar as it calls for a more precise definition of the criteria of the category so that an object cannot be found in two different boxes. In terms of objectivity, it also relates to the descriptive power of the category insofar as it must allow many to use them in the same way. Finally, the categories must

be adapted to the subject in the sense that they must retain the main themes, but also be adapted to the size of these themes.

Departing slightly from Berelson's use of categories, I prefer Guillemette' s use of codes which are then transformed into categories. Guillemette writes that on a first analysis, the scientist should use 'in vivo' codes as much as possible and then later change them if necessary, to attain a higher level of complexity or to use the scientific community's vocabulary. This will then become with further analysis, "pragmatic" codes, that is codes that are at a higher level of abstraction (Beck, 1999; Corbin & Strauss, 1990; Glaser, 1992). Categories, Guillemette writes, will serve to group various codes on the condition that this group constitutes a higher degree of abstraction and not a mere classification[75].

This chapter covered a lot of ground. What I have been trying to articulate, in sum, is that my position as a researcher is not from no-where, and that it must therefore be taken into account. In the chapter that follows, I show the care I have taken to be as "objective" as possible. What this chapter is attempting to say, however, is that full objectivity cannot be guaranteed and that either one can take some measures can be taken to come as close as possible to this hoped for objectivity, or one accept her immanence and limitation and, therefore, the limitations of her own study. The analysis that follows is therefore greatly indebted to the concepts thus far mobilized (sensitizing concepts), and I will do my best to show just how much so in the discussion. It is also to this theoretical framework that I owe my interest to the tests – a specifically modern way to bind and organize the social.

Sensitizing concepts

Actant; Alienated Knowledge; Allocentric Perception; Androcentric Bias; Autocentric Perception; Chimerism; Contextual Empiricism; Diffractive Method; Dynamic Autonomy; Dynamic Objectivity; Embodied Objectivity; Emergent Fit; Ethnography; Grounded Theory; Ideology; Individuation; Ideological Definitor; Ideological Public; Intersectionality; Model of Action; Mosaicism; Outsider Within ; Science as Dominating Nature and Women;

[75] I have to admit that before I was able to reach this higher level of abstraction, however, I had to "merely categorize". This helped me make sense of the 250+ in vivo codes I had.

Sensitizing Concepts; Spontaneous Feminist Empiricists; Standpoint Theory; Static Autonomy; Static Objectivity; Strong Objectivity

Chapter 4 –Research Strategy and Description of Content

Research Strategy

In this chapter, after a brief review of what constitutes my *Research Object*, I explain the process by which I constructed my sociological data (in *Construction of Data*). I do this by describing the material I used, the relation of communication at play, and by putting this material in relation with the object of research. Once this is done, I move on to a *Content Description*, i.e. what is said of my research object in the chosen material. This allows me to propose an analysis that underlines the social facts and semantic universe found in chapter 5, *Thematic Analysis*.

Research Object

My research object up to now has been the representations of (gendered) bodies. More specifically, as we have seen in the introduction, this object was circumscribed to: The Representations of Gendered Bodies in and through the Olympic Movement According to the IOC's Medical Commission Between 1967 and 1972.

Data Construction

I visited the IOC archives for five days stating on July 15, 2018. There, after discussing my topic with the archivists[76], I was offered a list of documents she thought would be relevant to my research. This list can be found in ANNEX I. The documents I ended up consulting from that list can be found in ANNEX II. Time constraints made it that I simply could not scan all the documents at my disposition. This precipitated the decision of focusing on the Meeting Minutes and correspondence of, and reports read by, the Medical Commission between 1967 and 1972. Indeed, the meeting minutes are accompanied by the reports read or discussed by the Commission members during those meetings as well as letters exchanged between members,

[76] Karine, Laura, and Sabinne.

NOCs and IFs. Between 1967 and 1972, 17 meetings were held. All have been considered for this analysis[77].

In Lausanne, therefore, I scanned all the documents found in ANNEX II. Once back home, I filed the Pdfs into Zotero[78]. This allowed me to have a rapid overview of the files and be able to make preliminary notes pertaining to their relevance, and search them according to their date, author name, type of document, and archival source file. Once I was able to do this, I could select the letters, reports, and meeting minutes written between 1967 and 1972. This amounted to 85 letters, 4 press releases, 17 meeting minutes and 6 reports. I read these documents in their entirety[79].

Content Description

In the following, I describe the documents analysed with attention to the communication relationship as described in the previous chapter through Ross's concepts: communication definitors and receivers. In this description of the material, I have endeavored to stick to the bear minimum by keeping what was directly related to my object of research, i.e. the representations of (gendered) bodies by the IOC Medical Commission. In principle, this should mean excluding most conversations on the procedural or technical aspects of the meeting minutes. In practice, however, the relationship between representations of the body and the choices of procedure and technics is not so easy to untangle. I thought it outside of my competence and objective to decide which procedural or technical passages fitted the description. Hence, we find here some passages having to do with procedural or technical decisions.

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[77] 1) September 26 / 27th 1967 (Lausanne); 2) December 20th 1967 (Lausanne); 3) July 13 / 14th 1968 (Lausanne); 4) October 1st 1968 (Mexico); 5) October 12th 1968 (Mexico); 6) January 25 / 26th 1969 (Lausanne); 7) June 12th 1970 (Munich); 8) July 29th 1971 (Lausanne); 9) January 29th 1972 (Sapporo); 10) January 30th 1972 (Sapporo); 11) February 3rd 1972 (Sapporo) 12) August 21st 1972 (Munich); 13) August 23rd 1972 (Munich); 14) August 25th 1972 (Munich); 15) August 26th 1972 (Munich); 16) September 1st 1972 (Munich); 17) September 2nd 1972 (Munich). Where meetings extended over two days and are separated by a / and counted as one meeting, it means that there was only one transcript of the minutes for the two days produced.

[78] This took about 100 hours of painstaking work.

[79] I note this because I want to stress that I did not simply use the command "ctrl F" to locate the keywords that I might have thought relevant. I wanted instead to be able to contrast and compare the discussions on the gendered bodies and gender tests with the discussions on doped bodies and anti-doping methods and techniques. This proved satisfactory, something I will come back to in the discussion.

Meeting Minutes

The meeting minutes vary in length, ranging from two to 20 pages for a total of 100 pages between the 17 meetings. They are all typewriter-typed. Some come with the agenda of the day, some don't. Some have the signed presence sheets, others a list at the beginning, others nothing at all[80]. When possible, I have indicated in a footnote who was present at each meeting. A detailed list of attendance can be found in the annexes.

Meeting 1, September 26 / 27th, 1967 (Lausanne)

This first meeting is attended by ten members[81] and the agenda lists eleven points[82] distributed on five pages. Elements relevant to our analysis are found in four sections: 3, 4, 6, 7. The rest discusses procedures and topics on which the Medical Commission has to take a position, e.g. the statutory rest period for certain athletes and the placement of doctors at the marathon.

In section 3, we read that sex-tests will be administered to the three finalists of each discipline and that doping tests will be administered at random. This decision is changed in *section 6*. *Section 4* discusses the legality of the tests. Dr. Genin declares that "the Organizing Committee in Grenoble will have no legal difficulties in connection with the testing mentioned in the entry form, due to the paragraph whereby the athletes agree already beforehand to subject themselves to medical tests"(Medical Commission 1967a, 2). He is probably the one intervening on this matter because he is the French representative and, therefore, I presume, the most familiar with the local laws. This being said, the French legal definition of sex is not discussed.

[80] In these cases, I complied the presences in the excel sheet found in ANNEX III based on the names of people intervening in the debate. This is obviously an imperfect method for some people may have been present without intervening, but was the best I could do.

[81] Prince Alexandre de Mérode (Belgium), Prof. Giuseppe La Cava (Italy), Dr. Roger Genin (France), Prof. Dr. Ludwig Prokop (Austria), Dr. Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), Dr. Alex Vecheler (Switzerland), M.J.W. Westerhoff.

[82] 1) Welcome 2) Roll Call 3) Basic working procedure 4) Medical control and test in connection with the new entry form 5) Completion of drug and dope list 6) Organisation of sampling procedure and protocol of anti-doping control 7) Contacts with press, radio and television 8) Statutory rest period for individual athletes taking part in successive events in boxing, wrestling and fencing 9) The marathon 10) Aid to athletes from developing countries 11) Duration of training periods at altitudes.

Section 6 is where the heart of the debate on gender and anti-doping tests is presented. In contrast to doping, where athletes are selected at random, it is said that gender-testing has to be administered to all athletes. This says something of the rationale of the tests and how the body is understood in both cases. Prof. Prokop suggests that the gender-tests be made on the three finalists instead of at random. This is unanimously accepted. At this point, talks only refer to "saliva samples". Whether those samples will be used to test chromosomal or chromatin composition is not mentioned here. Though it had been decided in *section 3* that the tests would be performed on the three finalists of each discipline, Dr. Hay then mentions that all 2000 athletes meant to compete in the women category could be tested. The meeting minutes do not mention if this is accepted, but we know from the reports that this was ultimately the chosen method for the Mexico OG[83]. As for the Grenoble winter OG, we know that 1/5 athletes were tested at random – not by their ranking positions. This whole section, though it is apparently meant to discuss both antidoping procedures and sex controls, is mostly devoted to anti-doping. Out of almost two pages for this section, 300 words are devoted to sex controls.

From *section 7*, we note the interest in orchestrating habituation in the athletes tested as we can read: "Press releases and interviews should be given in such a way as to avoid any impression of secrecy. The information should be distributed as widely as possible so that people, and more especially athletes, become accustomed to the idea of dope and sex tests" (Medical Commission 1967a, 4).

Meeting 2, December 20th, 1967 (Lausanne)

The meeting is attended by nine members[84] and the minutes are divided into five sections[85] on four pages. In principle, the minutes are also followed by annexes that go up to VIIb though I could only find annexes I, II, VI, and VIIb, in separate files. At the time of this meeting, the Grenoble OG were a little over one month away, as they opened on February 6, 1968.

[83] See meeting 4.
[84] Prince Alexandre de Mérode (Belgium), Dr. Roger Genin (France), Prof. Dr. Ludwig Prokop (Austria), Dr. Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Prof. Arnold H. Beckett (England), Dr. Alex Vecheler (Switzerland), M. Arpad Csanadi (Hungary), General Reinderhoff, M.J.W. Westerhoff.
[85] I. Sanctions, II. Doping: Method of Control III. List of Products considered as doping IV. Taking of samples. V. Choice of the sports event and the athlete to be tested.

In *section I*, it is noted that "the athlete who does not attend for the control will be disqualified"(Medical Commission 1967b, 1). *Section II* discusses the methods to be used for doping. In *section III*, which deals with the list of products considered as doping, of relevance to the representation of bodies, we can read: "We have concentrated on products which are detrimental when used by healthy athletes in competition, but which on the other hand are used for therapeutic reasons"(Medical Commission 1967b, 2). We will come back to why this is relevant in the analysis. *Section IV* discusses the methods of control for doping.

The first page of *section V* is devoted to procedural choices for doping control and the second page to sex controls. The procedures followed for sex tests are written as follow: "1) Sexual chromatins 2) Should a further examination prove to be necessary, a chromosome test would be carried out 3) If an even more complete examination is necessary, we would resort to taking a blood sample and carry out an F.S.H. count" (Medical Commission 1967b, 4). The presence of "further examination" is a development compared to the brief mention of "saliva" tests in September 1967. It also reflects a complexification of the representation models on the body, something we will come back to in Chapter 5 in the section on the Polymorphic Body.

Then, in a subsection of section V, titled *Choice of Athletes Tested*, we find out more about the reasons for this particular type of tests as well as on the procedures. We read:

> Bearing in mind the high cost of these tests and the facilities of the laboratories, we have suggested testing one female athlete in five, in such a way as to assure ourselves of these facts and avoid any unnecessary scandal. In principle, we have the intention of testing all the female athletes taking part in the Games following this system, according to the capacities of the laboratory. An Olympic certificate which will be valid for future competitions, will be given to female athletes after the control. In the event of some irregularity being found, the result of the control will be given only to the responsible medical officer of the team concerned, and to the President of this IOC Medical Commission or his representative (Medical Commission 1967b, 4).

I note the discussion on female bodies (to be tested or controlled, and given a certificate), the possibly scandalous nature of some results, the possibility of some irregularity, and the intention to test all female athletes.

Meeting 3, July 13 / 14th, 1968 (Lausanne)

The meeting is attended by ten members[86], the meeting minutes makes up five pages and is followed by six annexes, for a total of 52 pages. Annexe I is titled "Report on the Drug Testing Programme (Cycling Event) Fifth Pan American Games" and written by Dr. M.F. McInnes and J.W. Steele, both from the university of Manitoba. Annexe II is titled "Report by Doctor Thiébault on the Grenoble Games to the International Olympic Committee Medical Commission". Annexe III is the form for the Certificate of Tests of Sexual Chromatin. Annexe IV is the form for the Certification of Anti-Doping Tests. Annexe V is Pr. Beckett's Proposal on Dope Testing. Annexe VI is titled "The Work of the Medical Commission". Of particular importance in these annexes for our object is the presentation of Dr. Thibault's report on the Grenoble JO in Annexe II which will be seen in the "report" section (report #4).

The minutes are divided into seven sections: 1) Welcome by the President, 2) Roll Call, 3) Approval of the Minutes of the last meeting, 4) Correspondence, 5) Report on Grenoble by Doctor Thiébault, 6) Methods of Control in Mexico, 7) Conclusions. Points one to four and seven are of no relevance to our object for reasons made obvious by their very title.

In Point 5, we can read

> Certain criticisms had been made against the French on the following points… an insufficient number of female athletes had undergone the sex tests (50 out of 200)…Further, the President and Dr. Thiébault pointed out that a great many of the female athletes had been terrified of the sex tests as they had no idea what they consisted of. As soon as they learnt what the method used was that of buccal smear tests, the doctors had experienced no more difficulty (Medical Commission 1968a, 3).

Further, we read "a certificate that the sex tests have been performed will be sent to all athletes who underwent these tests in Grenoble, and a similar

[86] Prince Alexandre de Mérode (Belgium), Prof. Giuseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Dr. Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), Dr. Alex Vecheler (Switzerland), M. Arpad Csanadi (Hungary), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France).

certificate will be given to those who undergo the control in Mexico. This certificate will prevent any athlete from having to undergo a second test during any other future international or national competitions.

In Point 6 we can read :

> Dr. Hay remarked that as the majority of athletes would arrive in Mexico a month before the start of the events, the tests would easily be completed before the opening of the Games. Every athlete, without exception, would be tested. This test will be registered on a form such as the one outlined in Annexe III.

Meeting 4, October 1st, 1968 (Mexico)

Both October 1968 meetings are in the same document and start with Dr. Hay's General Report on the "practical dispositions that the [Organizing] Committee"(report #5[87]) had prepared, followed by the meeting minutes and a "Report Submitted by the Medical Commission to the General Session of the International Olympic Committee", the bulletins to the delegations, and some annexes containing the tests certificates.

Meeting 4 is attended by ten[88]. If we take out the list of presence, the October 1st minutes are two pages long and concisely cover twelve points including greeting and closing notes. Of the ten other points, one – point three — is on gender testing alone and two on gender testing and anti-doping controls (points two and eleven).

Point two is of little relevance to our object for it only states that Dr. Hay presents his report and that the practical dispositions are accepted by the Commission. *Point three* reads: "the President of the Commission informed that the Athletics, Fencing and Volley Ball Federations would register their athletes only when the Medical Commission certified that the results of sexual chromatin tests were satisfactory"(Medical Commission 1968b). This is

[87] See report 5 for precision about which report was presented then.
[88] , Prince Alexandre de Mérode (Belgium), Prof. Dr. Ludwig Prokop (Austria), Dr. Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France).

interesting because it conveys doubts coming from the delegations concerning the chromatin tests for the first and only time in the minutes.

Point eleven reads: "The President informed the Commission that the 16 International Federations present at the Meeting in Versailles, showed their entire confidence in the aims and methods approved by the Medical Commission". I could not find this Versailles meeting. Given what was said at point three, however, either the Athletics, Fencing and Volley Ball Federations were absent or point eleven refers only to the federations' confidence regarding doping controls. This said, it seems unlikely that three big federations such as Athletics, Fencing and Volley Ball were absent so most likely this point refers only to the aims and methods concerning doping and the doubts about the chromatin tests still stand.

Meeting 5, October 12th, 1968 (Mexico)

The meeting is attended by nine[89]. On page two, we read that out of a total of 848 athletes, 65 had not taken the tests as of Saturday October 12th, day of the opening of the games. In report #5, however, we are told that positive results were obtained for 803 athletes. Further, the Mexico games report of 1968 notes that there were 844 women athletes (Organizing Committee of the 1968 Mexico Games 1969, 165 of Volume II). What's more, though the Medical Commission reports states that all received their certificates (i.e. no one was declared not a female), we see on the official website of the IOC that 781 women athletes participated (IOC 2019). See report #5 for hypothesis about why this might be. The next day, "a group headed by Dr. van Dijk would be at the women's section of the Olympic village to make more sex determination tests from 9 to 12 hours"(Medical Commission 1968b). We hence learn that the Medical Commission was in charge not simply of designing the tests, but also of orchestrating them during the OG.

[89] Prince Alexandre de Mérode (Belgium), Prof. Giuseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Jacques Thiébault (France).

Meeting 6, January 25 / 26th, 1969 (Lausanne)

Meeting six has nine attendees[90]. The meeting minutes have six pages and two annexes (three and two pages respectively) for a total of 11 pages. I could only find the annexes in French. The first one is the letter addressed to General Thofelt (suede), president of the International Union for Modern Pentathlon from Prince de Mérode. The second is about the Dutch cyclists accused of having used "hormonoïde" substances. Neither are important for our purposes.

Regarding the meeting minutes, they mostly discuss the Swedish cyclist disqualification based on alcohol test. There is a strong push back against the alcohol testing, (point four and five are entirely devoted to that and to sketching a response to General Thofelt, president of modern pentathlon). The meeting is also revealing in terms of how open the commission is to outside opinion. At first, they seem to welcome collaboration, involving General Thofelt in the decision-making process, but then are really protective when being questioned on their methods. A question on their method is equivalent, in this meeting, to questioning the commission's "good faith" as we can see from this citation.

> To sum up, we had agreed, for the modern pentathlon, to include the alcohol tests in our rules but according to our methods. Therefore, it was entirely our responsibility, and an attack on our results meant that our good faith was called into question, which is inadmissible (Professor Beckett)

The Commission claims that the methods are questioned "at every turn" (Here, either Dr. Hay or Mérode is speaking; it's unclear). We learn, through a very revealing passage on the state of knowledge on hormones, that the ICU forbids the use of hormonoïde whereas the IOC does not, on the basis that they have little information.

> A debate then started on the methods of hormone dosage. Certain hormones are easy to detect, especially if they are synthetic, but the dosage of others is unknow or very difficult; even if their quantities are measured exactly, it is almost impossible to deduce the idea of fraud, an abnormally high dose with regard to the average will merely be noted; with horses, for instance, five times the normal rate is considered suspicious. (Professor Beckett).

[90] Prince Alexandre de Mérode (Belgium), Prof. Giuseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Jacques Thiébault (France).

> In men, and especially in women, physiological variations are such that it is practically impossible to fix an average rate (Dr Hay). The rates of each person should be known at least six months before the competitions (Prince the Mérode).
>
> In any case, if the ICU had the goodness to inform us of the hormone, and particularly the anabolism raters, which it considers dangerous, it would be of great help to us (Professor Beckett).

Meeting 7, June 12th, 1970 (Munich)

Meeting seven, has 15 attendees[91]. The meeting minutes are six pages long. The first five pages are spent discussing a booklet to circulate information about the anti-doping procedures. No such booklet is mentioned for gender testing. On this topic, we find, on page six, the suggestion that the IFs issue a uniform certificate. We also learn that certificates issued during "any world or continental championships" shall be accepted.

Meeting 8, July 29th, 1971 (Lausanne)

Meeting eight, has six attendees[92]. The three pages of minutes are all on the writing and printing of the brochure on doping control for the Munich Summer Games and the Sapporo Winter Games.

Meeting 9, January 29th, 1972 (Sapporo)

Meeting nine has 17 attendees[93]. The 15 pages of minutes of this meeting are on doping.

[91] Prince Alexandre de Mérode (Belgium), Prof. Giuseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Dr. P. van Dijk (Netherlands), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Mr. W. Daume (Germany), Mr. H. Kunxe (Germany), Dr. Hein(Germany), Dr. Käfer (Germany), Mr. Knoesel (Germany), Dr. Hegels (Germany), Dr. Donicke, Dr. Hauck, Mr. Kroppenstedt.
[92] Prince Alexandre de Mérode (Belgium), Dr. Ludwig Prokop (Austria), M. Arpad Csanadi (Hungary), Dr. Käfer (Germany), Dr. Hegels (Germany), Mr. A. Takac.
[93] Prince Alexandre de Mérode (Belgium), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Amilcare Rotta, M. Leclef, Bert Isatisch, Dan Steler, Dr. Dedic, H. Roos, Dr. Raas Ernst (Austria), J. Hederen.

Meeting 10, January 30th, 1972 (Sapporo)

Meeting ten is attended by ten[94]. The five pages of minutes of this meeting are on doping.

Meeting 11, February 3rd, 1972 (Sapporo)

Meeting 11 attended by nine[95]. The meeting minutes have four pages. We find out that the methods of the gender tests will be included in a booklet for distribution to the athletes. We also find out that Dr. Hay is suggested as the "specialist in genetics" by Prince de Mérode though his specialization is actually gynecology. This may explain why the proposition is declined and it is decided instead to send the "Danish professor in genetics" the booklet the MC is preparing.

> Mr. Vind had told Prince de Mérode that a Danish professor in genetics had contacted him and asked for information on the sex control. Prince de Mérode asked the Commission whether it considered the name of Dr. Hay should be given, as the specialist in genetics, or whether the Danish professor should just be sent the booklet giving the method of control. It was decided that this professor should be sent the booklet and informed that many experts are consulted on this matter.

I could not find the specific letter, but I could find the follow up exchange in which the Danish doctor in question, with four colleagues, sends a Memorandum to the MC via the Danish CO dated February 18 1972. This is Report #6 which will be analysed bellow.

Meeting 12, August 21st, 1972 (Munich)

The meeting is attended by 14[96]. Out of the five pages, about 175 words are on gender testing. Some competitors were coming in to be checked without their certificates. Many delegations considered that if their competitors had the certificates, a control was unnecessary. The only sports where a control was no necessary were yachting, shooting and equestrian, since

[94] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR).
[95] Prince Alexandre de Mérode (Belgium), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Prof. Arnold H. Beckett (England), Dr. Yoshio Kuroda (Japan), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR).
[96] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico),), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada).

women competed on the same basis as men. If anyone did not have a certificate, even though they said they had been checked, a new control would have to be made.

On anabolic steroids:

> Prince de Mérode informed the members that he had received a letter from Dr. Owen stating that although anabolic steroids should not be available, it was printed that they would be obtainable from the Organizing Committee on request. Dr. Käfer explained that many doctors required anabolic steroids for treatment of heart diseases. If they were used a month before the test was made, it would not show up. This was one of the reasons why it was important for the team doctors to submit a list of medicines, so that the Commission could know exactly what had been used. Dr. Hanley considered that it was very dangerous to have two different rulings; one in the IOC brochure and one in the Munich brochure. He thought it should be recommended that this drug be taken off the list of available medicines. Dr. Käfer said this would be too difficult since many other drugs would be involved. If this medicine was used for treatment, it could not be taken off.

Meeting 13, August 23rd, 1972 (Munich)

Meeting 13 attended by 14[97]. . Of the five pages, 180 words on gender testing, dealing with procedural matters, namely that an athlete, before she could compete in an event, needed to provide, on top of her ID card and Munich card, her femininity certificate.

Meeting 14, August 25th, 1972 (Munich)

The meeting is attended by 18[98]. The meeting minutes are tree pages long and focus mainly on the discussion on the use of Ventolin for asthma. I transcribe the passage on this issue because it deals with the representation of sick or abnormal bodies by the Medical Commission.

> Professor Reindell did not agree at all and considered that anyone who was asthmatic should not be allowed to participate in the Olympic Games. This

[97] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada).
[98] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico),), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Hegels (Germany), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada), Colonel Grut, Pr. Montanaro, Pr. Schonholzer.

should be considered as doping and therefore not permitted. In reply, Professor Beckett pointed out that this was a difficult problem since if everyone with a slight abnormality could not take part in the Olympic games, it would be ridiculous. The most important thing was to deal with the drugs that were deliberately taken to enhance performances. Professor Prokop thought it was not necessary to give sympatico-memetic drugs for asthma, and Dr. La Cava expanded that this competitor would have a definite advantage since he participated in the sprint events. Dr. Dirix pointed out that if the Commission agreed to this request, it would set a dangerous precedent for all other countries. After further discussion, it was decided that the Bahamas team doctor would be informed that since this substance was on the list of forbidden drugs, its use would be considered as doping, and therefore it could not be permitted.(Medical Commission 1972b, 11).

Meeting 15, August 26th, 1972 (Munich)

Meeting 15 is attended by 14[99]. One page of minutes for this meeting, only on doping.

Meeting 16, September 1st, 1972 (Munich)

Meeting 16 is attended by 16[100]. Of the six pages of these minutes, 100 words concern gender-testing. The rest is about the use of tranquilisers with the UIPMB, an Austrian weightlifter who had been found guilty of doping with the Austrian delegation, a member of the Dutch cycling team who was found to have been doped with the Dutch delegation. The discussions on gender focuses on a North Korean Volleyball athlete. We do not know from the minutes what exactly is the issue with her but we are told that the Volleyball Federation said that it was not their responsibility to check the identity cards, but rather should have been done by the Organizing Committee in Munich. The North Korean delegation said to Prince de Mérode that this affair was political discrimination and that if anything was done, they would withdraw from the Games. It was agreed that nothing could be done until an official protest was lodged by one of the teams. From the letters between the Volleyball Federation and Mérode, I could make out that the North

[99] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada).
[100] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico),), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Hauck, Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada), Pr. Kaarlo Hartiala.

Korean athlete had been suspected of having been replaced by another athlete for the gender-tests. I also learned in the letters that it was then decided, as a result of this controversy, that no femininity certificate would be issued to any volleyball athlete for this competition.

Meeting 17, September 2nd, 1972 (Munich)

Meeting 17 is attended by 16[101]. The minutes of this meeting are all on the question of *doping and the conflict with Colonel Grut and Pr. Schonholzer of the Munich Medical Commission.*

Summary

With regards to gender-testing, the meetings themselves are mostly concerned with the logistical aspects of the tests. The more philosophical discussions can be found in the reports I'll present bellow. Four out of the 17 meetings do not discuss gender-testing at all whereas all discuss anti-doping strategies and decisions. When gender-testing is discussed, it is considered a lot less abundantly than anti-doping is. I also note that the FI and CNO delegates only intervene on questions of doping – never on the question of gender testing. The discussion related from meeting 16 is a discussion that is summarised from the letters, not one that was debated with the delegates present in the Commission meeting.

Communication Relationship

There are 42 **definitors** of the Medical Commission meetings. The smallest meeting was composed of six people on July 1971 and the largest was of 18 on August 25th, 1972. The meeting mode and median is 10 and the average is 12. All of them, except one, are men. Most of the Commission members and FI and CNO delegates are European with the occasional Canadian, American, Mexican, Japanese, Russian and Puerto Rican delegates. Most of the Commission members were not permanent members of the IOC. Again, the attendance detail of the Commission's meetings can be found in ANNEX III. The author of the minutes, although his name is not found in all meetings, is general secretary of the IOC (J.W. Westerhoff). We know this

[101] Prince Alexandre de Mérode (Belgium), Prof. Guiseppe La Cava (Italy), Prof. Dr. Ludwig Prokop (Austria), Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico),), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), M. Arpad Csanadi (Hungary), Dr. Yoshio Kuroda (Japan), Dr. Herbert Reindell (Germany), Dr. Jacques Thiébault (France), Dr. Daniel P. Hanley (USA), Dr. Nina Grashinskaia (USSR), Dr. C. Laurin (Canada), Mr. Jones, Mr. Rieckehoffs.

through letters addressed to him complimenting him on the detail of the minutes (De la Cava 1968).

The Medical Commission Members

There were seven MC members at the first meeting[102] as well as the president of the MC (Prince Alexandre de Mérode), the vice president (Arpad Csanadi), and the general secretary of the IOC (J.W. Westerhoff), for a total of 10 attendees (Medical Commission 1967c). Dr. Genin and Dr. Vecheler seem to have stopped attending the meetings after this. The two doctors seemed to only have been officially replaced in January 1972 by Dr. Daniel P. Hanley and Dr. Nina Grashinskaia (Medical Commission 1972). In the meantime, Dr. Thiébault, who had started out as a representative of the Grenoble Winter Games was made a permanent member. Between 1968 and 1972, therefore, there were five permanent members, plus Dr. Thiébault who can be understood as a *de facto* member from the 1968 meeting on, for a total of six members, not counting the president and vice president and the general secretary, Westerhoff. The Medical Commission was very clear that Dr. Thiébault would be the last representative of an organising committee to be made a permanent member after there were discussions about a soviet and American doctor to join the Medical Commission. It was deemed unfair that each country would have a representant. So, Dr. Thiébault was selected on his merit and not on his being a representant of his country, which was to be the criteria for any further Commission member.

After 1972, with Dr. Hanley and Dr. Grashinskaia, there were thus eight doctors for a total of 10 permanent members with Prince de Mérode and Mr. Csanadi. Dr. Grashinskaia is the only female doctor and member of the Commission during the period analysed[103] and is also the only women to attend the meetings. Her first appearance is on January 29th, 1972 at the Sapporo meeting. This means, therefore, that of all the IF and CNO representatives, there were no women delegates between 1967 and 1972.

[102] Prof. Giuseppe La Cava (Italy), Dr. Roger Genin (France), Prof. Dr. Ludwig Prokop (Austria), Dr. Albert Dirix (Belgium), Dr. Eduardo Hay (Mexico), Dr. P. van Dijk (Netherlands), Prof. Arnold H. Beckett (England), Dr. Alex Vecheler (Swiss).

[103] Thanks to Miragaya, I know of Alice Millat, president of the women's sports federation in the 1920, and of Mireille Berlioux attending the EC meetings in the 1960s. Berlioux will become Director for the Press and Public Relations of the IOC from 1969 to 1985.

The selection process of the doctors constituting the Medical Commission is obscure. Apart from a mention of the Drs. being selected on their "competence and qualities" (Medical Commission 1969, 9) and archival letters from Prince de Mérode nominating some Drs and them accepting their nomination, it is hard to say on what criteria, selection process, or through which networks they come to the attention of the IOC. We know that Dr. Thiébault and Dr. Hay were selected as part of their country's delegations for the Grenoble and Mexico OG respectively and then remained on the Commission. We also know that Dr. Prokop was on the Executive Committee of the FIMS in the early 1960s. This may have been how Prince de Mérode heard of him. Dr. Dirix conducted some of the first doping tests on Belgian cyclists in 1965 and the first anti-doping tests at the Tokyo Games in 1964, after having lobbied Prince Mérode about the dangers of doping. Dr. Beckett (England) was known for his research on doping. As for Dr. Hanley (US), and Dr. Grashinskaia (USSR), they were nominated by their countries, assumedly their respective CNOs. I could find no information on the nomination process of Prof. Giuseppe La Cava (Italy), Dr. Roger Genin (France), Dr. P. van Dijk (Netherlands), and Dr. Alex Vecheler (Swiss). This could be a worth-while endeavour for further research.

Of the members mentioned above, only Prof. Dr. Ludwig Prokop attended all meetings. The ones who were the most present after him, that is with an attendance of 14/17 meetings, were Dr. Dirix, Dr. Hay, and Dr. Beckett. Dr. Thiébault was present at 13/17 meetings (or 13/15 meetings he could have attended because of his later arrival into the Commission). Dr. Giuseppe La Cava and Dr. P. Van Dijk were present from the beginning and at 12/17 meetings. Because of their higher attendance and the fact that they presented reports to the Commission and intervened more often, the doctors who had the most influence in the early direction undertaken by the Commission, among the 8 doctors presented, were Dr. Prokop, Dr. Dirix, Dr. Hay, Dr. Beckett and Dr. Thiébault. To better understand the communication relationship, therefore, I propose a brief overview of these five Dr.'s specialties. I do not discuss Mr. Csanadi and Prince de Mérode because neither have any medical training.

Dr. Hay (unknown - 2005)

was the son of a prominent Mexican politician of the same name (Guerrero 1993). He was Professor of Gynaecology and Obstetrics at the National University of Mexico City. He was also Director of the American British Cowdray Hospital in Mexico City and Director of the Amparo Maternity Hospital also in Mexico City. In the meeting minutes of the meeting of February 3rd 1972, we learn that he is the closest to a genetics expert the Medical Commission has (Medical Commission 1972a, 9). How the above curriculum accounts for this expertise is unclear. We have seen that Dr. Hay was selected as part of his country's delegations for the Mexico OG.

Dr. Prokop (1920-2016)

became professor of physiology and sports medicine at the University of Vienna in 1959 and became Director of the Institute of Sports Science in 1969 which he held until 1983. From 1979 to 1981, he was dean at the Faculty of Law and Integrative Sciences. He went on to earn his D.Phil. (Philosophy) at 73, a Dr.rer.nat. (Science) at 76 and a Dr.rer.oec. (Social and economic sciences) at 81. He was said to be an active swimmer, fencer, and pentathlon athlete. He wrote over 800 publications, including 32 books and 150 experimental works. In sports medicine, he founded the Austrian Society of Sports Medicine of which he was president. He was also president of the International Society for Sports Medicine (FIMS) from 1976 to 1980 and was on the Executive Committee of the FIMS in the early 1960s. He would go on to take part in 27 Olympic Games (as a team doctor and then doping expert of the International Olympic Committee). He is reported to have said that "sport is not meant to make you live longer but die healthier" (APA, 28.7.2016; *DER STANDARD* n.d.).

Dr. Dirix (1915 -1999)

was known for this knowledge of doping, published a dozen articles on the matter and gave international conferences on the topic. He was a member of four ministerial commissions on anti-doping in Belgium from 1962 onward. He conducted, with the Ligue Vélocipédique Belge (LVB) the first doping tests on Belgium cyclists in 1965. With the Union Cycliste Internationale (UCI), he conducted the first anti-doping tests at the Tokyo Games in 1964, after having lobbied

Prince Mérode about the dangers of doping. He was made professor of sport toxicology at the Université Catholique de Louvain in 1975 (“Breves Sports” n.d.).

Dr. Beckett (1920-2010)

rose to prominence in the 1970s and 1980s when steroid use amongst athletes was banned from certain competitions. He participated in several high-profile investigations such as that of Canadian sprinter Ben Johnson who was ultimately stripped of his Olympic gold medal for testing positive for the drugs in 1988. After controversial rulings were delivered following the 1992 Barcelona Olympics, Beckett resigned from the Medical Commission and ultimately became an advocate for athletes accused of doping. Little is known of Dr. Beckett prior to the 70s, and so how he came to the attention of Prince de Mérode is unclear.

Dr. Jacques Thiébault

I could not find any information Dr. Thiébault. What we know from the archival documents consulted is that he was a representant of the French delegation for the Grenoble games and was later made a permanent member. In this capacity, he produced an important report on how the unfolding of the first gender and anti-doping tests in Grenoble.

All in all, what can be concluded from this brief overview is that it seems as though only one doctor had expertise in sex/gender questions, i.e. Dr. Hay. That said, since he specialized in gynaecology, it is unclear why he was deemed the genetics expert. Considering what we saw in Chapter 2 on the history of endocrinology and genetics, and what we will see in the report submitted by doctor Van den Berghe, experts in endocrinology and genetics were not hard to find since the field was striving and benefited from large state support. How to explain the absence within the Medical Commission of an expert in either field remains a question at this point.

The communication **public**, or actors on the receiving end of the communication relationship, are mostly the Medical Commission members themselves for the minutes serve internal institutional memory purposes. The information and decisions taken based on these meetings and minutes, however, are then be communicated to the Olympic movement, i.e. international sports federations (IFs), national Olympic committees (NOCs), IOC members,

athletes, participating countries and spectators. The minutes themselves, however, are meant to be read by the Commission itself and at maximum the IOC members. This in part influenced my decision to take the meeting minutes as a material. I hoped that, because they were intended *for* the Medical Commission as an audience, as opposed to, say, the Olympic charters, destined to reach a wider audience, they would not gloss over certain uncomfortable positions. Further, I expected more straightforwardness in the discourse than something which may be directed at the public. I also expected this from the reports and letters. This communication relationship is reflected by the fact that there are traces of debates.

Though the intended audience of the minutes may have been the Medical Commission members, the communication relationship at play in these meetings is one that involves more than the Medical Commission members. Indeed, since the meetings welcomed representants from the FIs and the CNOs, they are also involved in the decision making. A meeting in which these representant are particularly vocal is that of 1972 on the question of doping. It is worthy of notice that no such representant intervenes on behalf of the female athletes being tested nor are the technics put into question by them, at least in the meetings. These interventions can be found in the letters.

Reports

There were more reports in the archives than the 6 presented here, but I selected only those that mentioned sex testing, even if not much was written about it. The reports are between 20 and 40 pages, for a total of 100 pages. Three of them, the reports by Pedro Ramirez Vasquez, Dr. Hay and Dr. Thibault, are reports on the testing procedures put in place at the Olympics; the first at the 1967 International competition in Mexico, the second (report #4) at the 1968 Grenoble Winter Games, and the third (report #5) at the 1968 Mexico Summer games. There is also a 1967 report by Dr. Prokop defining the role of the IOC medical commission with regards to the sex and doping tests. The other two reports are from “external experts”. One is from a doctor acting as consultant: Dr. Van den Berghe, and another is an unsolicited report from five Danish doctors who oppose the sex tests. Here is the order of the reports presented: Report #1, by Pedro Ramirez Vasquez, July 1967; Report #2 by Dr. Prokop, 1967; Report #3 from Dr. Van den Berghe,

May 29th 1968; Report #4, by Dr. Thibault sur les jeux de Grenoble à la commission médicale du CIO; Report #5 by Dr. Hay; Report #6 by the Danish Doctors.

Report #1, by Pedro Ramirez Vasquez, July 21 1967

The report is titled "Proposal Submitted to the Medical Commission of the International Olympic Committee". It is four pages long with an additional page for a sample 'test notice' form. It is divided into three sections: 1) Doping 2) Results 3) Sex Determination. The first two sections are contained on one page and not relevant to our purposes. Section 3 is three pages long and we can read:

> For the study of sex determination as for other branches of medicine, it is rather the observation of abnormalities which have been most useful for studying the normal or physiological.
>
> In this particular case the Medical Commission must decide by going from the normal to the pathological.
>
> The medical or legal definition of sex itself is subject to various interpretations and because of this we must base ourselves on recent studies in order to reach a fair concept of the sex of the individual. According to modern classifications we are able to consider nine groups as follows: 1. External appearance of the genitals. 2. Internal differences in the ducts. 3. Gonad identification. 4. Endocrinological sex. 5. Genetical sex. 6. Nuclear sex.7. Chromosomic sex. 8. Physiological sex. 9.Social sex.
>
> It is therefore quite natural that the Medical Commission of the IOC must work under pressure and under attack, inasmuch as even the interpretation of sex is based on so many different concepts.
>
> The purpose of the IOC in naming its Medical Commission is precisely so that we may aid and advise it on technical matters related to medical problems within sports.
>
> The Medical Commission has searched for a simple and practical method which will [not] injure the pride or the natural modesty of the athletes and which will enable

us to discover unfortunate cases where sexual anomalies may be found, as well as others (fortunately few) where fraud has been attempted by presenting an athlete who is clearly superior in anatomic type to other female competitors at an event.

The finding of Barr's corpuscle or a sexual nuclear Chromatinic mass, constitutes a very important factor in the diagnosis, which very quickly and easily allows us to eliminate all definite cases. It also has the advantage of causing the athlete no physical or moral discomfort and is in addition a practical and economical method of obtaining the sample.

The Medical Commission has been unjustly criticized because this method is not exhaustive and intensified and the argument has been put forth that other methods apparently more complete exist. But the fact remains that the Commission has found and tested this method and been able to do efficient work in determining the sex during the past International Sports Competition held in Mexico City in October 1967, and also at the X Olympic Games in Grenobles and the results were highly satisfactory.

In Mexico where the Games of the XIX Olympiad will take place, a special locale has been prepared at the Olympic Village (Feminine Section) where the Medical Commission will carry out the sex determination tests as the athletes of the different countries arrive. Fifty tests daily will be carried out.

We do not believe it to be necessary to test more people every day because the athletes arrive one month in advance and the testing will begin as soon as the Olympic Village opens on September 12.

The President of the Commission will give the necessary instructions and Dr. Eduardo Hay will be able to practice this test form the first days of September and to have practically all of the results ready upon the arrival of His Highness the Prince of Merode, so that at the first meeting of the Medical Commission results obtained may be examined and decided upon (Vazquez 1967).

Report #2, by Dr. Prokop, 1967

The report, dated December 20th 1967, is three pages long and divided into four sections: dope substances, the carrying out of urine tests, choice of athletes to be tested, chemical analysis, and the duties of the IOC Medical Commission. Though sex tests are mentioned in the last section, "should positive results be obtained during the dope test or other questions, such as those pertaining to the sex tests for example, arise, [the Medical Commission] should meet as soon as possible"(Prokop 1967, 3), nothing on our object, the representation of bodies, can be found. The discussion focuses on the selection of which drugs will be tested, the reasons certain drugs can be controlled or not, and the role of and protocols to be followed by the Medical Committee during the testing.

Report #3, by Dr. Van den Berghe, May 29th 1968

The report, requested to Dr. Van den Berghe by Dr. Dirix is sent to Prince Mérode on May 29th 1968. It is probably read by the Commission members before the following meeting in July though we cannot confirm this. The report, titled "A short survey of the more frequent sex aberrations and their causes", is 15 pages long. It is divided into three main parts: Normal Biological Sex Determination, Abnormal Biological sex differentiation, and How to Diagnose Genital Anomalies. It opens with the following remark : "it should be kept in mind that the sex of an individual is ultimately determined by the interaction of biological, socio-cultural, and philosophical parameters and that each of these can supply the cause of a deviation from normal femininity or virility"(Dirix and Van den Berghe 1968, 1).

The first part, NORMAL BIOLOGICAL SEX DETERMINATION, has three sections: 1) Genetic sex determination 2) Gonadal sex 3) Genital sex. It ends with the following comment:

> From this brief survey it appears that sexual differentiation follows a very accurate pattern but that before the biological sex is ultimately fixed the embryo is already 7 months old, so that there is ample time for that differentiation to be altered fundamentally… As a last point it should be remembered that at the time of puberty the hormonal changes which occur in the pituitary gland and the gonads will cause the secondary sex characters to

> appear, thus completing the male or female phenotype. Many a time it will be only during that period that an anomaly going back to embryogenesis will become apparent (Dirix and Van den Berghe 1968, 2).

The second part, ABNORMAL BIOLOGICAL SEX DIFFERENTIATION, has four sections: Sex-Chromosomal Aberrations, Gonadal Development Disorders, Developmental Disorders of Internal Genitalia, and Anomalies of External Genitalia. Hence, we see that the "normal body" does not discuss internal genitalia whereas "abnormal bodies" can be differentiated based on "abnormal" internal and external genitalia. The four parts are divided in the following fashion:

I. SEX-CHROMOSOMAL ABERRATIONS

1) Sex-chromosomal aberrations [ab.] in male phenotype individuals

A) Numerical ab.

i) Numerical X-chromosomal ab.

a. Klinefelter's syndrome

b. Patients with Karyotype 49 XXXXY

ii) Numerical Y-chromosomal ab

a. Patients with karyotype 47 XXY

b. 3 or 4 Y chromosomes

iii) Numerical X and Y-chromosomal ab.

B) Structural ab.

C) Mosaic patterns

2) Sex-chromosomal ab. in female phenotype individuals

A) Numerical ab.

i) One X-chromosome is missing; karyotype 45, XO

ii) More than 2 X chromosomes

B) Structural ab.

i) Deletion of the shorter arm

ii) Deletion of the longer arm

iii) Deletion of both arms

C) Mosaic patterns

D) Sex-chromosomal dysfunction

E) Sex-chromosomal ab. in patients with hermaphroditism

There, of interest to our analysis, we can read “more often than not a genetically male individual is likely to hide behind a female phenotype. On the contrary, a male phenotype in a genetically female individual will be found only very rarely” (Dirix and Van den Berghe 1968, 3). In the first “C) Mosaic patterns”, we read “a sex-chromosomal anomaly is not necessarily present in all body cells but may be limited to one or several tissues. In the other cells there can be either a normal chromosomal set or another chromosomal aberration” (Dirix and Van den Berghe 1968, 5). The second section, II GONADAL DEVELOPMENT DISORDERS, is divided into three sub-sections: 1) Gonadal agenesis 2) Gonadal Dysgenesis 3) True hermaphroditism. The third, III DEVELOPMENTAL DISORDERS OF INTERNAL GENITALIA, has no subsections. The fourth is divided in the following way:

IV ANOMALIES OF EXTERNAL GENITALIA

1.Infantile female genitalia
2.Adult female genitalia
3.External female pseudo-hermaphroditism
   a. Congenital cortico-adrenal hyperplasia
   b. Maternal androgenic influence
   c. Non-adrenal female pseudohermaphroditism
   d. androgen-producing growths
4.External male pseudo-hermaphroditism
5.Equivocal external genitalia
6.Simple male hypogonadism

In this section, we can read the following passage of interest:

> A complete review of all anomalies of the external genitalia would exceed the scope of this survey…it would appear [however] that the male genitalia are a further development of the female genitalia: the clitoris grows to a penis with a simultaneous shift of the urethral meatus toward the top of the penis.

The third part, How to Diagnose Genital Anomalies, lists the six ways of diagnosing anomalies available at the time: 1) (patient) History 2) Clinical Examination and Anthropometry 3) Hormone determinations 4) Sex-Chromatin or Barr-Chromatin 5) Karyotyping 6) Laparoscopy and Biopsy. In "History", we can read : "A thorough questioning of the patients and their families with regard to pregnancy, birth, and puberty will already provide useful information as to the causes and appearance of particular aberrations" (Dirix and Van den Berghe 1968, 13). In "Clinical Examination and Anthropometry" we read: "A thorough general examination of the patients and a detailed examination of both the genitals and the secondary sex characters will determine the degree of hypogonadism or pseudo-hermaphroditism" (Dirix and Van den Berghe 1968, 13). In "Hormone determinations", we read: "determinations of serum and urine estrogens and androgens are a yardstick for the patient's hormonal balance" (Dirix and Van den Berghe 1968, 13). In "Sex-Chromatin or Barr-Chromatin" we read: "by this very simple technique it is possible to form an idea of the patient's X-chromosome number" (Dirix and Van den Berghe 1968, 13). In "Karyotyping", we read: " The lymphocytes obtained from 2ml of venous blood are put into a special culture medium. After about 3 days most cells are actively dividing so that a large number of chromosome slides can be easily prepared. After these chromosomes have been photographed and the pictures enlarged the different chromosomes are cut out and pasted together in a given order based upon the size and position of the centromere. The Karyotypes assembled that way immediately point to any existing chromosomal aberrations. By incorporating radioactive nucleic acid precursors it is possible to investigate the DNA- synthesis of the different chromosomes, so that even disturbances in chromosomal function can possibly be evidenced"(Dirix and Van den Berghe 1968, 14). In "Laparoscopy and Biopsy" we can read "examination of the internal genitalia is possible only through laparoscopy. This will tell about the nature of the internal genitalia, whether there are any anomalies of the uterus and fallopian tubes, and whether gonads are present or not. Laparoscopy will also enable a biopsy of the various tissues to be made under direct visualization and microscopically to determine the correct nature of the tissues present. Integration of all those data will in most cases lead to a correct diagnosis, the first step toward adequate therapy"(Dirix and Van den Berghe 1968, 14–15).

Report #4, by Dr. Thiébault 1968

Found in meeting minutes of meeting 3, July 13th / 14th 1968, the report is 23 pages long and titled "Rapport sur les jeux de Grenoble à la commission médicale du CIO ». It is divided into two main sections: "I) Exposition of Motives and "II) The Methods Employed". Both sections are themselves divided into two topics: Sex control and Anti-dope control. Note that in Section II "sex control" becomes "the research into femininity" and "anti-dope control" becomes "The anti-dope struggle". "The research into femininity itself is separated into four sub topics: Research of the Barr corpuscles; Karyotype Research; Doubtful diagnosis; Conclusions. "The anti-dope struggle" is divided into six sub-topics: 1) Material used; 2) Reception of samples at the laboratory; 3) Communication of the results; 4) Analytical method used; 5) Number of samples analyzed; 6) Conclusions

In "I) Exposition of Motives" section under "a) sex control", which takes up about a page and a half from page one to two, we find a justification for the later change of "sex control" to "research on femininity". Dr. Thiébault writes:

> Above all this term, [sex control], gives rise to some confusion because it applies only to the female sex and because the idea of sex is itself psychologically quite variable. This is why I prefer the term 'research into femininity'. This has the advantage of …introducing the word 'femininity', which describes the characteristics peculiar to women without conjuring up precise anatomical traits (Medical Commission 1968a Annexe II, p1).

Later in this section, Dr. Thiébault, after mentioning the press' role into the push for gender testing states a second reason which is the "protection" of women's medals. He write: "It is inevitable that sooner or later the real representatives of the weaker sex will feel persecuted and will demand that their feminine records be attributed to them" (Medical Commission 1968a Annexe II, p1). He writes more about the motives, complexity, and orientation of the tests when he writes:

> this method [of sex control] was unnecessarily dangerous and even rather inhuman, as the scandal arising from the discovery of a mistaken sex would inevitably cause a host of scandalous headlines in bad taste...it is inhuman because in the case of dope there is an evident attempt of fraud whereas with feminine control all that is done is to confirm a default in nature, and it is for these people to plead that during their

> unadapted lives they had, thanks to sport, attempted a difficult [insertion] into an often hostile and cruel society[104] (Medical Commission 1968a Annexe II, p2).

Hence, we see that the very idea of the tests was not shared by all members of the Commission. Since he is nonetheless forced to go on with the tests, Dr. Thiébault once again insists on the moral duties of the Doctors performing these tests:

> our duty as doctors must prevail over all other things, even if they were Olympic, if we find such hybrid beings we must treat them if possible, or at least help them to accept their lot as we do when we discover any infirmity…the more or less radical sporting measures which result must be inspired by the desire not to harm but rather to help. Therefore our action will never be punitive but will always be dissuasive (Medical Commission 1968a Annexe II, p2).

In b) anti-dope control, from page two to page four, we also find causes for the anti-doping tests to be criticized. Dr. Thiébault writes:

> The third criticism most frequently expressed is that the list of stimulants and of evident toxicity includes normal medicines used for mild ailments and in weak doses, […] and this makes sportsmen and even their doctors say that we are showing an exaggerated intolerance as there is a world of difference between doctoring a slight cold and doping. However, here too the dividing lines are psychologically very difficult to trace, the more so as reactions to medicines often vary considerably. (Medical Commission 1968a Annexe II, p3).

A similar motive as with the gender tests pushed Dr. Thiébault to dissociate from police like methods: "also we did not believe in having to search the runners' luggage or clothes as these methods are the responsibility of the police and not of the doctors" (Medical Commission 1968a Annexe II, p4).

In "Methods Used" under "A) The Research into Femininity", is from page four to ten. The dissuading motive of the tests is there reaffirmed: "above all to dissuade the 'hybrids' from competing in the Games…it is in this frame of mind that the research into femininity must be done, by a scaling process designed to affirm totally the diagnosis of sex …" (Medical Commission 1968a Annexe II, p4). Then, Dr. Thiébault moves on to an explanation of the test to be performed (Barr corpuscle) and the following test after an unsuccessful Barr test, i.e. Karyotype test.

[104] The French resembles something more like :"…all that is done is to confirm a default in nature, and that, for beings to be pitied, for they will be, their lives during, unfit, and probably only attempted, through sport, to integrate, not without difficulty, a society who is often hostile and stupid."

> The presence of chromatinian corpuscles is exclusive to the female sex and will be evident whenever there are two chromosome X, as the cell of a normal man does not have them…for the female sex to be confirmed by this examination, a minimum of 20 cellular nucleus out of 200 carriers of the Bar corpuscles must be counted…the reading of the smear must be entrusted to a practiced biologist as the causes of error are not negligible: insufficient calculations, cells in pyonosis which cannot be interpreted […] If […]the number of Barr corpuscles counted is insufficient, the investigations must be continued taking good care [not to fix a][105] diagnosis.. this [lengthy] reminder of laboratory techniques is meant to draw attention to the relative complexity of this examination…here again…the biologist must be a perfect master of his technique and know how to read a karyotype, which is not always easy (Medical Commission 1968a Annexe II, p4-6).

Should that test also fail, he recommends that:

> further investigations should be carried out and a real hormonal [assessment of the athlete should be done]: the dosage of FSH, the dosage of the 17 cetos and 11 hydroxysteroids, study of the menstrual cycle, and in the very difficult cases, [we might consider relying] on the [study of anatomical and psychic characteristics] ... This complete valuation proves by its complexity and perspective which embrace all of the human characteristics that to want to consider it within the limits of the OG is an impossible scheme (Medical Commission 1968a Annexe II, p6).

Then, he repeats the main aim of the test which is to "not towards a pure and simple elimination of the incriminated sportswoman, but above all towards effectively helping the poor girl as much morally as medically" (Medical Commission 1968a Annexe II, p6). Later in that same section, Dr. Thiébault also comments on the "absolute proof of femininity" and hints towards why Barr tests are chosen over this "absolute proof", something we will to come back to in the discussion.

> It is obvious that maternity is absolute proof of femininity. We decided not to use it at Grenoble because the difficulties of investigation in an international circle are often large and the difference between a uterine child and one that has been adopted becomes extremely hard to determine. It is preferable to carry out unnecessary examination when required rather than embark on research into civil status which would be too risky. (Medical Commission 1968a Annexe II, p6).

After this, Dr. Thiébault, answering to some critics of the tests which condemn them because some athletes participating at the IOC are very young and could be traumatized by such tests, remarks that it would actually be beneficial for the tests to be conducted as early as possible:

[105] The sentence is as follow: "taking good care to keep the position on the diagnosis", however, a review of the original French report reads "se garder de prendre position sur le diagnostique" which is closer to what I have translated above.

> [A]t the age of puberty and pre-puberty, the sexual chromatin or karyotype are amongst the most sure examinations for the determination of sex, and I think that to detect any sexual inequality early, that is to say very young, could only be of serve, and I cannot see what obscure reasoning could find it preferable to discover a hybrid sex at the age of 20 rather than 13 years old. On the contrary it seems to me that at 20 everything is 'fixed' and that it is then very late to act, whilst at 13 treatment is still possible and above all, it is not too late if it is psychologically necessary to help to re-establish a person in his true sex. On the other hand then it is essential for the parents and doctor to be informed and that they themselves should take their child's interest in hand, and the latter ought to be told, at least, of any possible occurrences with great diplomacy and tact (Medical Commission 1968a Annexe II, p9).

The section, "B) The anti-dope struggle" extends from page ten to page 19. Sections 1, 3, 4 and 5 and are of no interest for the object of this study since they discuss the material used, the communication of the results, the method used, the analytical method used and the number of samples analyzed. In sub-section "2) Reception of samples at the laboratory", however, we note an importance given to authenticity, i.e. the correspondence between the sample and the body to which it belongs.

> "The authenticity and unimpaired condition of the samples were guaranteed by: A four-figure number engraved on the glass flask representing the identity of the sportsman who had been tested and the date when the samples had been taken. B) A red wax seal, making it impossible to open the flask without damaging the seal, which had on it the same number as that engraved on the flask and also the seal of the President of the Medical Commission" (Medical Commission 1968a Annexe II, p11).

In conclusions, numerous important remarks which depict a conception of the body or of gendered differences can be noted. First, this passage on the difference of techniques and hence results between experts of different countries highlights the fact that bodies, as data, can be interpreted in various ways, something we will see in chapter 5. Indeed, Dr. Thiébault notes:

> One thing was clearly apparent from this experiment: the toxicological experts worked in their own countries with different equipment and procedures, and from this arose the first misunderstanding, because to begin with it leads to a very disagreeable atmosphere of suspicion, and it is not natural that the IOC Medical Commission should be used as a battle ground for quarrels between experts –this is neither the desire of this Commission not, even less, its purpose….it must be mentioned that laboratories which are experienced in the technical analysis of stimulants are still very rare; to our knowledge the only ones which could have helped us are in Ghent, Rome, and London. (Medical Commission 1968a Annexe II, p15).

A second important conclusive remark is one on the gendered differences in terms of exposition to doping tests. Dr. Thiébault writes: "one must also take into account the emotional effects and

the work should be done as calmly as possible bearing in mind the modesty of the sportsmen and especially that of the women… I think I can say that it was the first time that anti-dope controls had been practiced on women in France" (Medical Commission 1968a Annexe II, p16). Finally, another gendered difference is the closing question of the report: "should we recommend every kind of sport to our [women], and are the women's sports what they should be?" (Medical Commission 1968 Annex II, p19).

Report #5, by Dr. Hay 1968

Dr. Hay's report is added to the document "General Report on the Work of the Medical Commission of the International Olympic Committee during the Games of the XIXth Olympiad", supposed to have been presented during the October 1st 1968 meeting, but is not the report mentioned in the minutes. I come to this conclusion because this report clearly speaks of the Games in the past tense. The Mexico Olympics took place between October 12 to 27 1968. Hence, by October 1st, they had not yet taken place. What is more likely is that a shorter version was presented at the October meeting and that this report was added to the General Report after the games.

In any case, Report #5 is 15 pages long. It is not very well structured for it starts with three points 1) General Organisation 2) Transportation 3) Office Facilities to jump on an unnumbered "SEXUAL CHROMATIN" section which lists one point "1) MATERIAL AND METHOD" and then another unnumbered "Microscopic Examination" section which has four points. We then have the "RESULTS" section and the "DOPING" and "SPECIMENS" sections where numbering seems to have been abandoned completely to be replaced by capitalized and underlined titles, whereas numbering seems to have been reserved to lists. We then have a "DISCUSSION AND COMMENTS" section and a "CONCLUSION".

Of the 15 pages, the discussion and comments take up four pages, the gender-testing takes four and doping controls seven. In the four pages on gender testing, the procedures followed are described precisely up to the microscope used and names of the chemists analysing the samples. We are told that positive results were obtained for 803 athletes, though the Mexico games report of 1968 notes that there were 844 women athletes (Organizing Committee of the

1968 Mexico Games 1969, 165 of Volume II) and though Medical Commission reports states that all received their certificates, we see on the official website of the IOC that 781 women athletes participated (IOC 2019). These discrepancies can reveal that some athletes may have been silently "pushed" into retirement or non-participation. This would require further research.

Report #6, Danish Doctors, February 18 1972

Report #6 found in the letter sent to the IOC on February 18 1972 by Gudmund Shack and signed by five Danish doctors is seven pages long. It is titled "On the Use of Sex Chromatin Investigation of Competitors in Women's Divisions of the Olympic Games". And has five sections: The definition of sex; The disagreement between different definitions; The "sex test"; The consequences of using the sex test in the way proposed by the Olympic Committee; Conclusion.

<u>The definition of sex</u>

This section is divided into three subsections: A. Chromosomal sex B. Somatic sex C. Psychosocial sex.

A. Chromosomal sex : There, after a cursory explanation of the chromosomal sex, we can read : "it should be stressed that a distinction has to be made between chromosomal sex and genetic sex, which is a broader term, as genes on the autosomes are also likely to play a part in the sex determination of humans, but very little is known about the role of autosomal genes in sex determination"(Shack 1972, 2). I will come back to this in the analysis, but it is interesting to note that knowledge that the genetic make-up of sex determination exceeds the karyotype was already present in 1968.
B. Somatic sex : This is said to be determined by the gonads, the genital organs and the secondary sex characteristics.
C. Psychosocial sex: Psychosocial sex is defined as "identical with the gender role of the individual. What is important is which sex the individual believes himself or herself to be, and this determines the sexual behaviour and role (male or female) which he or she wants to play in society" (Shack 1972, 2). I will also come back to this in the analysis.

<u>The disagreement between different definitions</u>

Here the "disagreements" between the different definitions are discussed, e.g. a disagreement between chromosomal and somatic sex, seen "in individuals with Morris' syndrome (testicular feminization)" (Shack 1972, 2). The section ends with this acknowledgement of trans people: "there are individuals with a total disagreement between the chromosomal and somatic sex on the one side and the psychosocial sex on the other" (Shack 1972, 3).

The "sex test"

Detailed description of the how the sex chromatin investigation is done. They conclude

> it immediately becomes evident that a sex chromatin test does not tell very much about the chromosomal sex, indicating only the number of X chromosomes present. It tells us nothing about the presence or absence of a Y chromosome and nothing about the structural abnormalities of the sex chromosomes. Therefore, the sex determined by this test…may disagree not only with the somatic and psychosocial sex but even with the chromosomal sex. (Shack 1972, 4).

The consequences of using the sex test in the way proposed by the Olympic Committee

In this section we read:

> The frequency of sex chromatin negative women is about 0.5 per thousand, and the following examples should be mentioned: a) ovarian dysgenesis b) pure gonadal dysgenesis c) testicular feminization (Morris' syndrome)…we consider these examples sufficient to show that if sex chromatin investigations are used in the way proposed by the international Olympic committee, a number of women would be excluded from the Olympic and other international sports game for no acceptable reason. This is a discrimination against a minority which is made even more unreasonable by the fact that in groups a) and b) (see above) some women are excluded and others not, although the clinical pictures are identical (Shack 1972, 4–5).

In "Conclusion", we read:

> The psychosocial sex is firmly established in early childhood, and a later change in the psychosocial sex can only be obtained at the expense of severe psychological disturbance in the individual… To use the test in the way proposed by the international Olympic committee, i.e. in women and not in men, is a discrimination against women in general; and use of the test in both sexes would involve that e.g. 46 XX and 47 XXY males should compete with women (Shack 1972, 6).

Communication relationship

Since five of the seven reports are written by doctors of the medical commission, and those have already been discussed in the communication relationship of the meeting minutes, I will discuss

here the Dr. Vanden Berghe and the Danish doctors. First, it should be noted that when Dr. Hay is pointed to in the meeting minutes of February 3rd 1972 as the genetics expert, it is also said that many international experts were consulted. However, the only report made by an external expert before February 1972 is that of Dr. Vanden Berghe which I hope the reader can agree hardly counts as "many experts". More research should be done to find other reports if they exist, but given that I had access to all the archival files from this period and did not find any, it is unlikely that others will be found. Of the experts writing reports for the Medical Commission, therefore, only one was solicited and the others wrote in reaction to the measures already taken by the Commission. We also note that all of them are men and all but two are of European descent (the two others being Mexican).

Herman Vanden Berghe M.D. (1933-2017)

In Dr. Van den Berghe's obituary we can read :

> He was one of the founding fathers of VIB [Vlaams Instituut voor Biotechnologie] and the inspirer and founder of the Center for Human Genetics in Leuven. We will remember him … for his groundbreaking research in the field of human genetics.("In Memoriam Herman Vanden Berghe | VIB" n.d.)

Another obituary of the British Medical Journal states that "when he returned to Leuven in the autumn of 1963, he opened the first human genetics laboratory in Belgium" (Stafford 2017). From this we can infer that genetics in 1968 was in its very beginnings.

Danish Doctors

The Danish doctors who sign the report #6 are:

<u>Erik Strömgren (1909-1993) M.D.</u>

Was professor of psychiatry University of Aarhus and an eminent professor of international reputation. We can read about him that he "is famous for his effort to raise psychiatric research to the same level as other areas in medicine". He was also linked to the WHO "where he among other things contributed to some of WHO international multicentre studies of schizophrenia" ("Erik Strömgren" n.d.). He founded an institute for research in psychiatric epidemiology and genetics, the Institute for Psychiatric Demography of which it is said that it "became a 'Mecca' for visits from all over the world" (Bertelsen 2009). He was also adviser to the

Danish governmental department in charge of the state mental hospitals for more than 20 years and an adviser to the West German government for the re-establishment of German psychiatry.

Johannes Nielsen M.D. (1924-2017)

He was the Chief of Service at the Cytogenetic Laboratory at Aarhus State Hospital and the Institute of Psychiatry University of Aarhus. He was "particularly interested in people with malformed sex chromosomes and in 1969 received his doctorate in a dissertation on Klinefelter's syndrome"("Johannes Nielsen" n.d.). He wrote or was co-author of 460 publications.

Mogens Ingerslev M.D. (1913-1992)

Professor of obstetrics & gynecology University of Aarhus from 1953. He later became the Dean of the Faculty of Medicine ("Mogens Ingerslev" n.d.).

Gert Bruun Petersen M.D. (1934-present)

Assistant professor of human genetics University of Aarhus. He received a gold medal for his research into chromosomes in 1962 ("Gert Bruun Petersen" 2016).

A.J. Therkelsen, M.D. (1924- present)

Professor of human genetics University of Aarhus and Head of the Institute of Human Genetics since 1971. Was also a member of the Danish Society of Medical Genetics (Wertz and Fletcher 2012, 476).

Press Releases

The press releases are about one page each for a total of 4 pages. Not all press releases are dated. When no date is given, I approximated the date according to context and/or archival source file which provided date ranges. In them, in contrast with the meeting minutes, everything is presented as a given and as established. The press releases' use is to inform the public of the decisions taken by the IOC, in this case regarding the procedures undertaken by the medical commission.

Letters

The letters date from January 3rd 1967 to September 8th 1972. They were taken from the following archival documents: BID04-Medic 034, BID04-Medic 001, BID04-Medic 035, BID04-Medic 038, JO1968W- Medic, BID04-Medic 019, BID04-Medic 020, JO1972W- Medic, and JO1972s Medic. The letters are one to two pages each, for a total of 150 pages between the 85 letters read. Of those 85 letters, I read all and determined that 36 were relevant because they either discussed sex testing or helped put together the timeline of the creation of the IOC. I will not summarise the letters here but say that, of those, two contained reports not found in the minutes and which to my knowledge have not yet been analysed by scholars of sex testing. The first report is Dr. Van den Berghe's report, found in Dr. Dirix's letter of May 29th 1968. The second is a report signed by five Danish Drs and found in the letter of Danish Olympic Committee president, Gudmund Shack. Those are analyzed in the section on Reports. Otherwise, the letters are either from the president of the IFs to Brundage, or between Westerhoff and the Medical Commission members. There are two instances of a member of the public writing the IOC: one journalist and one student. 90% of the time, the letters we easy to identify since the dates, sender, and recipient well indicated. For about 10% of the letters, no name was provided for the sender. That said, I could usually recognize the signature from a previous letter sent by the same author, or by identifying the letter head of the organisation and searching who was the president of the organisation for that year. Apart from a few hand-written exceptions, all letters are also typewriter-typed.

Chapter 5 – Analysis

Thematic Analysis

This chapter presents the thematic analysis emerging out of the meeting minutes, reports, and letters exchanged by the IOC's Medical Commission. There, the discourses about the body can be separated into two main categories. First, a category concerning how the body is conceptualized emerges. Second, a category about the relationship between the body and the Medical Commission appears. The first category can be further dived into three types: *The polymorphic body*[106], *The "normal"(dimorphic) body*, and *The "abnormal" body*. The second category, similarly, can be divided into three categories as well: *The body as a site of intervention*, *The body as scientific object*, and *The body as object of ethical concern*.

The body	The relationship between the body and the MC
The polymorphic body	The body as site of intervention
The "normal" (dimorphic) body	The body as scientific object
The "abnormal" body	The body as object of ethical concern

The body

We saw in chapter one that there are various ways of thinking about the differences between bodies. The main ways presented in that chapter were to think of the differences as dimorphic (as characterized by the biomedical sciences) or as the result of a bicategorization (as characterized by the social sciences). In this section, I grapple with these different ways of thinking about the differences across bodies in relation to the language used in the texts analysed. By

[106] This category is called *the polymorphic nature of the body* and not *the normal body as polymorphic*, which would have been symmetrical with the category of *the normal body as dimorphic*, because the question of normality does not necessarily emerge in these discussions on the body being polymorphic. It is as if the polymorphism is taken for granted as a base and then the discussion of abnormality is superimposed when it comes to think of sex differentiation. The texts do not focus so much on defining what is normal. Instead, we are left to understand the "normal" by negative definition of what is defined as "abnormal".

doing so, I came up with three categories: *The Polymorphic Body*, the *"normal"(dimorphic) body*, and *The* "*abnormal" body.*

The Polymorphic Body

Throughout the reports and meeting minutes, sex determination is attributed to various sources, given different actors in charge of its definitions, and even discussed in terms of degrees. The sources of sex determination include anatomic traits, biological functions, chromosomic and genetic composition, gonadic and genital (internal and external) type, hormonal quantities, social, psychological, legal, and even philosophical considerations. The result is that, at times, the human body, and particularly it's classification into two sexes, appears anything but given for the IOC's Medical Commission's doctors.

Indeed, Dr. Thiébault, in his 1967 report (report #4) writes "the notion of sex is in itself physiologically quite protean" (Medical Commission 1968a, 1, Annexe II) [107]. This position echoes the explanation given by Pedro Ramirez Vasquez's in his report (report #1) regarding the definitions of sex. There, we can read:

> The medical or legal definition of sex itself is subject to various interpretations …According to modern classifications we are able to consider nine groups as follows: 1. External appearance of the genitals. 2. Internal differences in the ducts. 3. Gonad identification. 4. Endocrinological sex. 5. Genetical sex. 6. Nuclear sex. 7. Chromosomic sex. 8. Physiological sex. 9. Social sex (Vazquez 1967, 2).

Report #3 by Dr Van den Berghe goes even further than Vazquez' suggestion of biological and social sex when he writes:

> Although the morphological aspect and the biological characters of the child obviously play an important part in legal sex determination, it should be kept in mind that the *sex of an individual is ultimately determined by the interaction of biological, socio-cultural, and philosophical parameters* (Dirix and Van den Berghe 1968, 1, my emphasis).

[107] « La notion du sexe est en elle-même assez protéiforme ». Unless otherwise specified, all translations in this chapter are mine. In sentences where a list of words is used, e.g. "it is explained, for example, by a *resistance* (to androgen), by "hormonal *changes*", by "*deviation*", by "*disturbance*", by "*disorder*", by "fundamental *alteration*", by "*failure* of nature" or by something being "*faulty*" or "imperfect"", the references to each word is cited in the same note.

Indeed, not only does Dr. Van Den Berghe add philosophical considerations[108] to the already mentioned biological and social aspects, but he further underlines the interaction between those three factors.

What is clear is that there are multiple factors that play into sex determination, some social, some biological. Within the biological factors, a multiplicity of determinants also exist. We will see that each determinant, be it chromosomal or anatomical, can also vary in intensity. Most importantly, however, the determinants can "contradict" one another, so to speak. That is, one can have a "male" chromosomic composition and a "female" genitalia, for example. This can be explained by a chromosomic "anomaly" like a XXY chromosomic composition. Even within this "anomaly", however, there exists much variation. Indeed, Dr. Vanden Berghe writes "a sex-chromosomal anomaly" like the one just mentioned, "is not necessarily present in all body cells but may be limited to one or several tissues. In the other cells there can be either a normal chromosomal set or another chromosomal aberration" (Dirix and Van den Berghe 1968, 5). i.e. another chromosomal anomaly, for example XXXY or still another. This is what we have seen in earlier chapters as mosaicism.

Beyond there being multiple factors to consider when discussing sex, the reports and minutes show an awareness of there being a variation in intensity within those very factors. Indeed, Dr. Hay, writing about anabolic hormones, notes that "in men, and especially in women, physiological variations are such that it is practically impossible to fix an average rate"(Medical Commission 1969, 5). Further, it is even stated that they cannot be "effectively measured" or that "reaction to drugs is often idiosyncratic"(Medical Commission 1969, 5; 1968a, 3, Annexe II). Even within the category of "true hermaphroditism", the variations are great, according to Dr. Hay. It includes "a whole range of patients with varying phenotypes. In most cases the karyotype was normal and in a few instances a mosaic pattern was found"(Dirix and Van den Berghe 1968, 8). This variation is also found in gonadic composition. Indeed, there are "many forms of gonadal

[108] It is hard to imagine what exactly is meant by philosophical considerations here. I can only assume that he means something close to some personal, psychological, beliefs. It could also mean teleological positions on the function of men and women. Further research might help clarify his definition.

dysgenesis in which chromosomal examination will be found mostly normal" (Dirix and Van den Berghe 1968, 8).

In Dr. Thiébault's report, cited above, it is further remarked that:

> In cases where the diagnosis is still uncertain, further investigations should be carried out and a real assessment of the athlete should be done: the determination of FSH, the determination of the 17 ceto and 11 hydroxysteroids, the study of the menstrual cycle and in the very difficult cases, we might consider relying on the study of the anatomy and of the psyche ... This real expertise proves by its complexity and its perspectives which include all of the human personality that to want to consider it within the framework of the OG is a utopia (Medical Commission 1968a, 5, Annexe II)[109].

This passage, as well as considerations coming up as early as Meeting 2 (Medical Commission 1967b, 4) of the need for additional tests on top of the chromatin tests (such as the chromosome tests and FSH count) reflect a complication of the representation models on the body. Indeed, apart from the fact that the chromosome tests require more advanced equipment and more time (cf Quail 2019) it takes into account the Y chromosomes. By doing so, it includes into its analysis bodies that have a XXY chromosome type and does not automatically exclude XYY types with conditions such as androgen insensitivity syndrome and gonadal dysgenesis, which leads to female phenotype, something that the chromatin test would fail to notice. Something similar is true of the FSH count.

In 1968, Dr. Moore, in a scientific article where he criticizes the IOC's decision to tests the athletes based on their nucleic sex, writes that there are "nine components of sexual phenotype (external genital appearance, internal reproductive organs, structure of the gonads, endocrinologic sex, genetic sex, nuclear sex, chromosomal sex, psychological sex, social sex)" and that it is therefore unfair to test an athlete on only one of these (Moore 1968). There is, in any case, a clear recognition of the "complexity of the subject". This complexity is such that Dr. Vanden Berghe notes in his report that he cannot review "all anomalies of the external

[109] "dans les cas où le diagnostic serait encore douteux, il faudrait pousser les investigations encore plus loin et procéder à un véritable bilan hormonologique de l'athlète : dosage des FSH, dosage des 17 ceto et 11 hydrosyxstéroides, étude du cycle menstruel et dans ces cas très difficiles, s'appuyer sur l'anatomie et l'étude du psychisme …Cette véritable expertise prouve bien par sa complexité et ses perspectives englobant toute la personnalité humaine que vouloir l'envisager dans le cadre des JO est une utopie.»

genitalia"(Dirix and Van den Berghe 1968, 10). Given this, and the fact that "the medical or legal definition of sex itself is subject to various interpretations", he argues that the objective is not so much to establish a stable definition but rather a "fair concept of the sex of the individual" (Vazquez 1967, 2). These "various interpretations" echo what is stated in the reports mentioned above, but especially that of Dr. Moore. Indeed, Dr. Vanden Berghe, besides writing about the interconnection of the social and biological factors, also writes that there are nine groups of sex determination according to the "modern classification": "external appearance of the genitals; internal differences in the ducts; gonad identification; endocrinological sex; genetical sex; nuclear sex; chromosomic sex; physiological sex; social sex" (Vazquez 1967, 2).

The "Normal" (Dimorphic) Body

Dear Mr. Westerhoff, In reply to your circular letter of the 31th October...although we would like to help you in this matter I am happy to say that our Federation to date has not experienced any particular problem in ascertaining the sex of its players since at the moment only males - generally of a particularly rugged type - participate in international competition (Palmer 1967).

The "normal body", though it may be understood as multifariously determined and even going through various sex determining phases, is also thought to culminate into the alignment of all or most of its sexual characteristics into one of the two sexes. Exemplifying this paradigm, Vanden Berghe writes, "biological sex determination distinguishes three main stages which each in succession can be the turning-point of normal sex determination" (Vazquez 1967, 2). To put it differently, there is a "decisive turning point" (Dirix and Van den Berghe 1968, 3) where the sex determination "ultimately fixes" the "final aspect" (Dirix and Van den Berghe 1968, 2) in the body's "completion" (Dirix and Van den Berghe 1968, 2). Hence, the "normally developed" body appears in one of the two dimorphic categories, for "sexual differentiation follows a very accurate pattern" (Dirix and Van den Berghe 1968, 2). This point is hammered in by statements such as "there are 2 sex chromosomes, vis XX in females, XY in males" (Dirix and Van den Berghe 1968, 13). This differentiation is "initiated" and "commanded" "by the chromosomes" (Dirix and Van den Berghe 1968, 1) and the secondary sexual characteristics are "induced by hormones" (Dirix and Van den Berghe 1968, 2).

The predominant idea here is that there are two sexes and that there is a correspondence between these ideals and the empirical: we can see this in statements that allege finding the "real sexe"(Medical Commission 1968a, 8) or the "real representants of the weaker sex" (Medical Commission 1968a, 1)[110]. Dr. Thiébault also speaks about "psychically [reinserting] a being into its true sex »(Medical Commission 1968a, 8)[111]. To contrast this notion of a true sex, there is also discussion of a "false sex" (Medical Commission 1968a, 2).

For a female body, the supreme proof of femininity, for Dr. Thiébault, is maternity:

> It is obvious that in the case of motherhood, the absolute proof of femininity is acquired. If we believed in Grenoble not to use it, it is because in the international environment the difficulties of interrogation are often important and that the differences between the child uterine and the adopted becomes extremely difficult to determine(Medical Commission 1968a, 5)[112].

Though (biological) maternity is the ultimate proof of femininity, the passage also recognizes, however, that filiation may not be conceptualized in the same way across individual or cultural idiosyncrasies. Hence, biology is presented as that which can settle cultural or individual disputes on a subject for it offers a simple and irrefutable answer. This also implies, however, that biomedical discourse or the answers proposed is somehow not affected by culture, something which we saw might be problematic according to the feminist epistemologists discussed in Chapter 3.

Other determining feminine characteristics include being of "short stature" (Dirix and Van den Berghe 1968, 5), "slim" (Dirix and Van den Berghe 1968, 9), having "breasts" (Dirix and Van den Berghe 1968, 4,7,9,10), two "x chromosomes" (Dirix and Van den Berghe 1968, 1,4,5; Medical Commission 1968a, 4, Annexe II), "the presence of chromatin corpuscles" (Dirix and Van den Berghe 1968, 14; Medical Commission 1968a, 4,5, Annexe II), "ovaries" (Dirix and Van den Berghe 1968, 2,10), "oestrogens"(Dirix and Van den Berghe 1968, 2, 13), a "vagina" (Dirix and Van den Berghe 1968, 2, 7), a "high-pitched voice"(Dirix and Van den Berghe 1968, 4), "menstruating"

[110] 1)« véritable sexe » 2) « véritables représentantes du sexe faible »
[111] « Réinsérer psychiquement un être dans son véritable sexe »
[112] "Il est évident que dans le cas de la maternité, la preuve absolue de la féminité est acquise. Si nous avons cru à Grenoble ne pas nous en servir, c'est parce qu'en milieu international les difficultés d'interrogatoire sont souvent importantes et que la différence entre l'enfant utérin et l'adopté devient extrêmement ardu à déterminer »

(Dirix and Van den Berghe 1968, 9,13), having a specific "fat distribution" (Dirix and Van den Berghe 1968, 4) and being "weak" (Medical Commission 1968a, 1, Annexe II)[113]. Whereas masculine attributes include "hairy" (Dirix and Van den Berghe 1968, 7,8), possessing "testis" (Dirix and Van den Berghe 1968, 7,8,10), a "wolffian system", and a "y chromosome" (Dirix and Van den Berghe 1968, 1,4,14). The possession of an extra Y chromosome as in chromosomal anomalies such as XYY is associated with height that "easily exceed 180cm" and "delinquent aggressive behaviour". They also possess some advantage (Dirix and Van den Berghe 1968, 14).

To which sex an individual belongs can be determined "about the 4th month [when] the sex of the embryo can be told from its external genitalia"(Dirix and Van den Berghe 1968, 2). Indeed, a big emphasis is placed on external genitalia: "the many variations in the different phenotypes must be considered within the frame of normal embryological development of the external genitalia"(Dirix and Van den Berghe 1968, 10).

This conception is different from the isomorphic conception presented by Laqueur in that even if the body is seen as complex and somewhat processual, the undertone is still that the person who does not fall into either category is in some kind of limbo. At best, according to the 'normal body' paradigm, the determination of sex should be based on as many factors as possible, but the objective is still to place the individual in one of the two groups.

The Abnormal Body

The following statement by Dr. Vanden Berghe on the relationship between normality and abnormality corroborates what I alluded to in this section's introduction as justification for their not being a stand-alone "normal" category like the "abnormal" category. Dr. Vanden Berghe writes: "it is rather the observation of abnormalities which have been most useful for studying the normal or physiological" (89:4). Indeed, this focus on abnormalities to discuss the normal is what comes out of both the reports and meeting minutes. A telling example is the fact that "aberration" is used in 13 segments, making it the most used noun of the documents (Dirix and Van den Berghe 1968, 1,3,5,7,8,13,14). This abnormality or aberration is explained in three

[113] "La présence des corpuscules chromatiniens est lié au sexe féminin, se trouvant présent chaque fois qu'il existe deux chromosome X puisqu'aucune cellule des individus masculins normaux n'en possède qu'un ».

different ways. First, in term of some lack, second, in terms of an error or "wrong path", and third in terms of limbo.

First, abnormality or aberration is most often explained in terms of some developmental lack. That is, the aberration is described in terms of something "disappearing" (Dirix and Van den Berghe 1968, 8), of something "missing" (Dirix and Van den Berghe 1968, 5,8) of some "loss" (Dirix and Van den Berghe 1968, 6), of some "deletion" (Dirix and Van den Berghe 1968, 5,6,9) of a something "shortened"(Dirix and Van den Berghe 1968, 5,6), of "dysgenesis" (Dirix and Van den Berghe 1968, 6,7,8,9,10,11) or "agenesis" (Dirix and Van den Berghe 1968, 8), both of which point to some failure to develop, by "incompletion" (Dirix and Van den Berghe 1968, 8), by an "infantile" (vdb6,11) state, which is to say, again, not fully developed. "Hypofunction" and "afunctional" are interesting ones since they seem to be located between the vocabulary of lack and that of malfunction or disorder which we will see below (Dirix and Van den Berghe 1968, 2,11).

Second, abnormality is also described in more active terms. It is explained, for example, by a "*resistance"* (to androgen) (Dirix and Van den Berghe 1968, 8), by "hormonal *changes*" (Dirix and Van den Berghe 1968, 2), by "*deviation*" (Dirix and Van den Berghe 1968, 5), by "*disturbance*" (Dirix and Van den Berghe 1968, 2), by "*disorder*" (Dirix and Van den Berghe 1968, 8,10), by "fundamental *alteration*" (Dirix and Van den Berghe 1968, 2), by "*failure* of nature" (Dirix and Van den Berghe 1968, 5) or by something being "*faulty*" (Medical Commission 1968a, 2 Annexe II) or "imperfect" (Dirix and Van den Berghe 1968, 10; Medical Commission 1968a, 3 Annexe II). This language of being defective seems to carry, by its use of "active" verbs, a presumption of will and therefore fault. We can see clearly how this fault is conceptualized in the following sentence in which the "abnormal" body is "hiding" another one: "more often than not a genetically male individual is likely to hide behind a female phenotype" (Dirix and Van den Berghe 1968, 3). This indeed seems to capture, in medical terms, the deception anxiety that justified the gender tests in the first place. The worry is that a man may be hiding behind a woman. The cytologists that discovered the XY and XX chromosomes, however, were interested in describing the process by which the sexes differentiate. The chromosomes, for them, initiated the march towards one or the other sex. Deciding of someone's sex based solely on their chromosomal organisation such

that the chromosomes prime on all other biological determinants, therefore, seems quite far from the cytologists' interpretation. To speak of chromosomes as hiding is even farther removed from their conception.

Third, abnormality is described in terms that echo a sort of limbo. The identification of this anomaly is made, there, through the "in between quality" of the bodies described (Medical Commission 1968a, 1, Annexe II). They are "abnormal" because they do not neatly fit into either one of the categories. They are "women built like halls of forts" (Medical Commission 1968a, 5, Annexe II), "hybrids" (Medical Commission 1968a, 5,8, Annexe II), "indeterminate creature" (Medical Commission 1968a, 4, Annexe II) with "no clear-cut secondary sex characters" (Dirix and Van den Berghe 1968, 8), "stuck" in some "intermediate stage" (Dirix and Van den Berghe 1968, 9) [114].

Relationship between the Body and the Medical Commission

We saw in chapter one that the scientific culture analysed here (1968 to 1972) is not the same as today's. Indeed we noted that Löwy wrote: "neither sex nor gender can exist outside of a society or culture, but the cultures that shape them are not necessarily the same. Today, biological sex is understood in the light of a specific culture: that of contemporary science and biomedicine" (Löwy 2003, 100). In this section, therefore, I try to bring to light what the scientific culture of the time had to say about their own role, here the medical commission, in relationship to bodies. If "biology is politics by other means" according to Fausto Sterling (Magubane 2014, 764), then how the medical commission saw its role vis à vis the athletes' bodies (its political positioning) is relevant to the biological discourse they produced.

Bodies as Scientific Objects

Throughout the documents, bodies are presented as sites of intervention or manipulation. In the present context, these manipulations have to do with the second relationship which is one of bodies as abstracted categories. In this relationship, the Commission's task is to classify the bodies. In order to so, however, they must first test and manipulate them.

[114] « Femmes bâties comme des halles des forts »

These tests and manipulations emerge throughout the documents in the seven turns of phrases used to refer to the tests. We find “contrôle (cliniques) *de* sexe” or “contrôle *du* sexe”(Medical Commission 1967b, 1,4; Beckett 1967, 1), “examens de féminité”/ “femininity test” (Medical Commission 1968a, 4, Annexe II; 1967b, 4), “investigation of sex », “recherches sur la féminité”, “sex verification”, “sexual chromatin examination” (Medical Commission 1968a, 4, Annexe II; 1969, 7). These all allude to a relationship between the scientific and his or her object of study, mediated by the tests. Namely, this relationship is one of a body to be “studied”, “tested”, “controlled”, “verified”, “examined” or “investigated”. Further the body so studied, tested, controlled, examined, verified or investigated is the feminine body as is made clear by the use of “femininity test”. The fact that “femininity” is used and not “female” might indicate a somewhat conscious limitation of scientists regarding their ability to determine what counts as female since femininity already carries more of a cultural connotation.

The conception of the body as an object of scientific discovery is also present through the discussion of the various types of tests used by the Commission. They discuss the use of "venipuncture", "blood test" "chromosome examination", “biopsy”, “photograph”, “clinical examination”, and “anthropometry”. Each of these tests imply a certain understanding of the body. What’s more, certain tests are considered « safer » than others because they cannot be counterfeited by human error (e.g. a photograph of the chromosomes is more reliable than a clinical examination). The goal of the Medical Commission, after all, is to “[remove] the doubt with absolute certainty” with regard to how to categorize a body (Medical Commission 1968b).

Further, it seems as though different conclusions can come from different types of tests, as can be seen by this passage:

> In cases where the diagnosis is still doubtful, further investigations should be carried out and a real honomonolgic assessment of the athlete should be carried out: the FSH dosage, the 17 ceto and 11 dydrozysteroid dosage, the study of the menstrual cycle and in the very difficult cases, rely on the anatomy and the study of the psyche (Medical Commission 1968a, 5).[115]

[115] « Dans les cas où le diagnostic serait encore douteux, il faudrait pousser les investigations encre plus loin et procéder à un véritable bilan honomonolgique de l’athlète : dosage des FSH, dosage des 17 ceto et 11 dydrozystéroides, étude du cycle menstruel et dans ces cas très difficiles, s’appuyer sur l’anatomie et l’étude du psychisme »

This seems to imply that the tests may reveal different results or that a body, when examined with one technique may express a certain characteristics, and when scrutinized by another technique, may express another. More concretely, a body that may reveal a female phenotype in a clinical examination may turn out to reveal a male phenotype under a chromatin examination.

Bodies as Abstracted Categories

Categories are formed by putting together objects which share at least one characteristic. They aren't "natural" objects. That is, if we encounter unmistakable differences in nature, the ways in which these differences are grouped is not given *in/by nature*. As such, categories are abstractions. Their content is abstracted from natural objects. This does not mean they are not real. The natural objects they describe, and the characteristics used to group them are real, but categories as such do not refer to any flesh and blood "thing-out-there". This is a classic nominalist point (Hacking 1999; Mackinnon, Maclagan, and Austin 1939; Wittgenstein [1921] 2001).

With regards to the human body, we have seen that sex is most often presented as a dimorphic category, meaning that there are two categories. We saw in the first chapter that the characteristics that make a body fall into one category or the other have changed through time. Further, the ways in which they have been agglomerated has also changed—at times external genitalia being the only thing considered, at other times, the gonads, or hormones. We also saw that which characteristics are considered or isolated from the body to place it into one or the other category has changed with the available knowledge and tools at hand.

Likewise, the Medical Commission, with the tests at their disposition, were are able to abstract certain characteristics of the bodies to categorize them. This is true for sex categories as well as for bodies in contact (or not) with dope. At first, the doctors they remain hesitant about the diagnosis, or placement into a category. They are incited to "continue the investigations, taking good care not to take a stand on the diagnosis" (Medical Commission 1968a, 4, Annexe II)[116]. This is also true of doping, of which the boundaries "are physiologically very difficult to trace" (Medical Commission 1968a, 3, Annexe II)[117]. Ultimately, however, the diagnosis has to be

[116] « Continuer les investigations en se gardant de bien prendre alors position sur le diagnostic».
[117] « Les frontières du dopage sont physiologiquement très difficiles à tracer ».

"final" (Vazquez 1967, 3; Medical Commission 1968a, 4, Annexe II; Dirix and Van den Berghe 1968, 13)

Through the tests discussed above, the Commission claims to be able to determine to which category a body belongs to: "the fact remains that the Commission has found and tested this method and been able to do efficient work in determining sex" (Vazquez 1967, 3). Once these categories are established, it can proceed to certify a body's fit into a category and thereby stabilize and institutionalise those categories:

> A certificate of control of sex will be addressed to all the athletes who underwent this control in Grenoble, a card of the same order will be issued to the athletes who will undergo this control in Mexico. This card will allow, during the subsequent international or national competitions, to avoid a second control"(Medical Commission 1968a, 3, Annexe II)[118].

The Commission remains in control, too, of the boundaries of the categories "the Commission reserved the right in special cases to decide on whether or not the above-mentioned certificates should be accepted" (Medical Commission 1970, 4) and act as an authority on the matter within the IOC which it "advise[s] on technical matters related to medical problems".

Category formation implies the exclusion of certain objects from particular categories. This abstract exclusion takes an immediately concrete form, however, since it is directly applied to the IOC regulations. In practice, this exclusion can take the form of eliminations or sanctions. Interestingly, the language of sanction only appears in the cases of doping, except in the case of an athlete not showing up for gender testing : "exclusion of the Olympics ... the athlete that does not present herself [to the tests] will be disqualified" (Medical Commission 1967b, 2)[119].

With the gender tests, the majority of the time, what is analogous to the doping "sanction" sections are a divulgation of the information to the International Federation: "in cases of irregularity, the president will summon the Medical Commission, the Doctor of the team and of the International Federation concerned" (Medical Commission 1967b, 3). The same is true for « disqualification », i.e. it is mostly used in the context of doping though the language of

[118] « Un certificat de contrôle de sexe sera adressé à toutes les athlètes qui ont subi ce contrôle à Grenoble, une carte du même ordre sera délivrée aux athlètes qui subiront ce contrôle à Mexico. Cette carte permettra, lors des compétitions ultérieures internationales ou nationales, d'éviter un deuxième contrôle».
[119] « Sanctions : exclusion des JO...l'athlète ne se présentant pas au contrôle sera disqualifié».

disqualification also appears in the 1968 minutes regarding gender testing, again in the case of a no-show to the test: “any female athlete who does not present herself before 11th October 1968 will be disqualified” (Medical Commission 1968b communique to delegations). Whereas in doping, the presumed fault justifies the sanction: “eliminated the competitor at fault” (Medical Commission 1969, 5). Sanction may be more easily given due to the fact that the categories are more stable, i.e. lists of prohibited drugs are circulated before the games (Medical Commission 1969, 5).

Though the “abnormal body” is not to be officially sanctioned, as the one who would use dope, it still is at least symbolically sanctioned through the strong dissuasion from participation to the OG. Indeed, we can find the following statements about dissuasion in the Meeting Minutes: “our action will never be punitive but aims at deterring” (Medical Commission 1968a, 2, Annexe II)[120]; “in Grenoble, the idea was to ... dissuade 'hybrids' from coming to the games” (Medical Commission 1968a, 4, Annexe II)[121]; or again “even before the Olympics, we learned from the press that two Soviet and one Bulgarian athletes had preferred not to come to Grenoble, which shows that our goal of deterrence had already been achieved"(Medical Commission 1968a, 7, Annexe II)[122]. In sum, the goal of the Medical Commission was not to punish “abnormal bodies” but to erase them, if not *tout court* (through “corrective therapies”), then at least from the OG.

Bodies as Objects of Ethical Concern

Whether out of concern for the bodily or mental health of their patients by offering a cure to “undetermined creature” (Medical Commission 1968a, 5, Annexe II) or to preserve the “purity” of the OG female category, the doctors of the Medical Commission understand their role as providing “moral and medical assistance”[123]. They are concerned with the “human rights” of their patients and also about their “social insertion”(Medical Commission 1968a, 2, Annexe II). In fact, this social insertion is one of the justifications for therapeutic interventions on bodies that do not

[120] « notre action ne sera donc jamais punitive mais restera toujours dissuasive”.
[121] « Il s’agissait à Grenoble de…dissuader les ‘hybrides’ de se présenter aux jeux ».
[122] « Dès avant les jeux olympiques, nous avons appris par la grande presse que deux athlètes soviétiques et une bulgare avaient préféré ne pas se présenter à Grenoble, ce qui tend à prouver que notre but de dissuasion était déjà atteint».
[123] « Aide morale et médicale »

neatly fall into either sex category. In that spirit, Dr. Thiébault writes, “we are mere observers of a failure of nature, and this, in beings to be pitied, for they will be, their lives during, unfit, and probably only attempted, through sport, to integrate, not without difficulty, a society who is often hostile and stupid” (Medical Commission 1968a, 2, Annexe II)[124].

These “abnormal” bodies are indeed subject to pity as can be seem by the following descriptors: “unfortunate cases” (Vazquez 1967, 3), “unfit” (Medical Commission 1968a, 2, Annexe II)[125], "to be pitied" (Medical Commission 1968a, 5, Annexe II)[126], "difficult insertion into society” (Medical Commission 1968a, 5, Annexe II)[127], “poor girl” (Medical Commission 1968a, 5, Annexe II)[128]. The doctors’ role, therefore, is to treat and protect them. Indeed, the Medical Commission is concerned that “no physical or moral discomfort” (Medical Commission 1968a, 7, Annexe II) should transpire from the tests. They are also wary not to “[traumatize] the psyche of teenage girls” (Medical Commission 1968a, 7, Annexe II) [129].

The therapeutic motive comes up several times. The diagnosis of an “undetermined creature”, “to judge of its state and to use possible therapeutic” (Medical Commission 1968a, 5, Annexe II)[130]. In Dr. Thiébault’s opinion, however, the sooner the intervention can be done, the better, for “at 13 years all therapies are still possible” (Medical Commission 1968a, 8, Annexe II)[131]. What’s more, finding out about this “error” of nature is better “it is essential that the parents and the child's doctor be informed and take the interests of their children in their own hands, the latter being made aware of as little as possible and with a lot of tact” (Medical

[124] « On ne fait que constater une défaillance de la nature et ce chez des êtres à plaindre qui seront leurs vies durant des inadaptés et qui probablement grâce au sport tentaient une insertion difficile dans une société souvent hostile et souvent bête. ».
[125] « Inadaptée ».
[126] “êtres à plaindre ».
[127] « Insertion difficile en société ».
[128] « Pauvre fille ».
[129] « Il semblerait que la crainte de voir traumatiser le psychisme de jeunes adolescentes soit le principal écueil ».
[130] « De juger de son état et d’user de thérapeutique éventuelles ».
[131] « Alors qu’à 13 ans toutes les thérapeutiques sont encore envisageables ».

Commission 1968a, 7, Annexe II)[132]. The goal of these treatments is to "psychically [reintegrate] a being into his/her true sex (Medical Commission 1968a, 8, Annexe II)[133].

Nonetheless, the "abnormal" bodies are not the only ones the Medical Commission feels "responsible" for. Indeed, they are very concerned for the "fair sex". For one thing, they wonder whether they should "advocate for any sporting exercise to our companions" (Medical Commission 1968a, 16, Annexe II) [134]. In other words, they wonder if "female sport what it should be?". And, for another, the tests' justification is primarily to dissuade and exclude "abnormal" bodies for the sake of insuring that « women [Olympic] records » be allocated to the "real representatives of the fair sex" (Medical Commission 1968a, 1, Annexe II). Hence, at times, the Medical Commission seems more committed to the protection of the "fair sex" from those who would allegedly have a certain competitive advantage over them. The "abnormal bodies", in this context, verge the category of "frauds".

That said, the Medical Commission is very clearly opposed to a specific kind of investigation, i.e. police investigation. If this first motive appears in Dr. Thiébault's report, " we did not think it necessary to ask that the possible search of the luggage or the costumes of the runners could take place, these methods falling under the police and not the medicine" (Medical Commission 1968a, 3, Annexe II)[135], more than once, "replace the word control which seems quite police-like" (Medical Commission 1968a, 1, Annexe II)[136], it is also found in the mouth of Prince Alexandre de Mérode elsewhere "under no circumstances would we accept denouncements…if we agreed to work in this way, an atmosphere of suspicion would quickly reign in the Olympic villages which would become vast police departments"(Medical Commission 1969, 5).

[132] « Par contre, il est alors primordial que les parents et le médecin de l'enfant soient mis au courant et qu'ils prennent eux-mêmes les intérêts de leur enfant en main, ce dernier devant être mis au courant du moins de choses possibles avec beaucoup de doigté et de tact ».
[133] « Réinsérer psychiquement un être dans son véritable sexe".
[134] « Doit-on préconiser à nos compagnes n'importe quel exercice sportif, le sport féminin est-il bien ce qu'il devrait être? »
[135] « Nous n'avons pas cru devoir demander que la fouille éventuelle des bagages ou des costumes des coureurs puissent avoir lieu, ces méthodes relevant de la police et non de la médecine».
[136] « Remplacer le mot contrôle qui a un aspect un peu policier».

Chapiter 6 – Discussion

Method

I start with some technical considerations on my method. First, I believe it was beneficial to keep a wide reading scope when reading the documents. That is, I did not limit myself to the sections that pertained only to gender testing such as one a reading guided by using “ctlr c” to look up certain keywords would result in. The reason I think this was beneficial is that I could compare the Commission’s treatment of gender testing to their treatment of the anti-doping tests. Doing so, I was able to see that they were less worried, when it came to gender testing, with the legal aspects and therefore careful elaboration of the procedures as they were in the case of the anti-doping procedures (Medical Commission 1969, 5,6). It seems the Commission was more concerned, in the case of gender testing, with the athlete’s well-being – perhaps because they realized the violence of their category formation.

Second, my initial coding contained codes to identify the sections that discussed gender testing from those that discussed anti-doping measures. I found these codes to be irrelevant in the end for they were too wide and whatever information I initially wanted to get from these codes, I could get from the more precise codes that accompanied them. Third, if I had to do this again, I would start filing the documents in my Zotero library at the archives. I may have been able to scan less documents this way, but the workload afterward would have been less overwhelming.

In Chapter III, I introduced Berelson's criteria, as presented by Sabourin, and I promised to deal with the adjustments I made through my work to meet these criteria. In the first step, therefore, my goal was to create categories that would be exhaustive and adapted to my material. That is, I have prioritized seizing as many details as possible, most of the time by creating in vivo codes. I wanted to retain words and formulations that are specific to the document. So, I started by creating a code for just about every word that seemed to allude to the body or the tests types. Having done two discourse analysis before, I was careful not to reproduce a previous mistake, i.e. having a “*Miscellaneous*" *category.* I was able to avoid having recourse to a miscellaneous

category by being diligent with the notes I took in relation to each code. For each segment with a specific code, therefore, I wrote down why I assigned it this code instead of another that may have had a close connotation.

I ended up with 273 codes. Then, with Atlas.ti's "group codes" function, I filed those codes into 10 groups: 1 - Communication of results, 2 - Function of the tests / doctors, 3 - Negative endogenic valuation of methods, 4 - Negative exogenic valuation of methods, 5 - Normality / abnormality, 6 - Paternalism / virtuous drs, 7 - Procedural / judiciary / legal discourse, 8 - Positive endogenic valuation of method, 9 - Sex differentiation, 10 - Test Type. After further reflection, I reduced these to four groups: 1 – Valuation of methods (now containing 3, 4, and 8) 2 – Function of the tests / Drs. (now containing 2, 6, 7, 10) 3- Sex differentiation 4- Normality / abnormality. This allowed me to build the analysis proposed in the following section.

Another detail worthy of mention, though it is less technical, is that I was very glad to find Dr. Vanden Berghe's report in the final days of writing this. To the best of my knowledge, this report has not yet been referenced by any of the secondary literature, which leads me to believe it may have been forgotten. A more thorough analysis of this very rich report would be highly beneficial for the understanding of the state of the knowledge on intersex people in 1968.

A further difficulty is that I seemed to be unable to stick to my research object. As a reminder, that object was the representation of the (gendered) body. Because I chose to look at this representation through the prism of gender testing, however, I was constantly brought back, in my coding, by the historical and technical elements of the tests in the documents. I do not think this harmed the analysis of my object; it only made this analysis longer and more complicated.

Theoretical considerations

First, some general thoughts about the categories that emerged. My categorisation may appear to suggest a contradiction in the Commission's understanding of the body by presenting the Commission as understanding the body as both polymorphic *and* dimorphic. This is not what I want to suggest. Rather, I believe that, though the reports seem to emphasize the complexity and perhaps continuity between the sexes, the Drs.' imperative to decide was prompted by the specific social circumstances in which they found themselves, i.e. the Olympic Games, which

called for a clear-cut differentiation between the sexes. That is, the imperative can come from problems that are not medical per se, but come, instead, from considerations that are outside of medicine or the biomedical sciences. Consider the following passage: “It is useless to dwell on the reasons which crystallized this question [of needing gender testing], the press and unfortunately very often the sensationalist one spoke at length of these so-called women built like forts of the halls and collecting records”(Medical Commission 1968a, 5, Annexe II)[137]. This emphasis on the sensationalist coverage seems to indicate that the demand for gender testing as of 1936 came from a social controversy, i.e. the media complaining about some women’s bodies, and not from a biomedical or scientific controversy.

This was also the case with the resurgence of the tests in 2009, with the media’s treatment of Semenya Caster. What’s more, and as we saw in Chapter 1, the social pressure to resolve some anxiety about “abnormal” bodies was also what prompted the research on homosexuality. In all of these cases, two things become apparent. First, a socio-political controversy is confounded for a scientific one. Indeed, there may have been no reason for scientists to have to “decide” on what counts as a woman or not were it not for this pressure from the IOC and the media. Second, it appears that a biomedical answer was sought out to deal with what is a strictly social problem. As Latour writes, "sciences are one of the most convincing tools to persuade others of who they are and what they should want" (Latour [1983] 1999, 35). It seems indeed that the tests have served this persuasive function with regards to what others are and what they should want.

Further, we saw that, though the first tests happened in 1936 during the Olympics, the IOC was not the first to impose them. Instead, the IAAF put in place the anatomic version of the tests in 1966. When the time came to integrate women into the track and field disciplines, it put into question the definition of women sport. The latter was, up until then, understood mostly as a mode of sociability, as we have seen in Chapter 2, where « equalizing » technologies were accepted to make up for women’s « natural weakness ». These « social » sports were also thought to be gentle enough that they wouldn’t jeopardize a woman’s reproductive capacities, and, with

[137] « Il est inutile d’épiloguer sur les raisons qui ont cristallisé cette question, la grande presse et malheureusement bien souvent celle à sensation s’est largement faite l’écho de ces soi-disant femmes bâties comme des forts des halles et collectionnant les records ».

luck, even favour them. When women started using only their bodies for sports and being interested in pushing back the limits of these bodies as well, anxieties creeped up. Were women starting to masculinize? Worst, when it comes to track and fields, since it was one of the first sport to professionalize, could we imagine that a woman would spend more time running than taking care of her family? That these considerations were behind the rationale of the tests is evident from the discussions around the participation of women in track and fields (Miragaya 2006).

The historical survey of women's participation in the Olympic Games of Chapter 2 shows that as of 1964, the question of eliminating the participation of women from the games is dropped. Three years later, the focus changes on to how to make sure women athletes are "real" women. One can't help but make the hypothesis that, having failed to eliminate women from participating in sports all together, and, further, having largely failed to keep them from participating in sports deemed "unfeminine", the focus was shifted to how to make sure the women who did participate in these sports remained "feminine". The tests come in as a possible answer to that since they contribute to eliminate from participation bodies that fall in between the two accepted gender categories. We also saw in Chapter 2 that the IOC chose in the 1920s to incorporate women's sports so it could regulate it instead of letting it be developed by Alice Millat, or any women. In so doing, and with the gender testing policies that discouraged women with stronger bodies, it may have curbed women's rate of physical performance.

Second, I mentioned in my theoretical framework that recursivity was fundamental to grounded theory. For this reason, here are some reflections on how my sensitizing concepts may have influenced my reading of the documents and subsequent categorization. Because of Laqueur's two paradigms, of which I was aware before starting this research, I have developed an interest and sensibility in looking at other people's categories in terms of whether or not they see continuity (isomorphism) or discontinuity (incommensurability). It seems the doctors of the Medical Commission swung between the paradigms of isomorphism and incommensurability,. We saw in Chapter 1 that deciding that "the real biological sex of an individual" ought to be declared based on their external genitalia (penis, vagina) or gonads (testis or ovaries) is not quite the same as attributing the chain of events of sexual determination to gonads or chromosomes. Both allow for sexual variation, or thinking outside the sex binary, but they do it differently. The

latter does so by admitting that, despite an initial determination, much can happen between it and the full phenotypic expression. There is nonetheless this 'initial determination' which is harder to get rid of. The former, on the other hand, allows for variation by adopting a much more conventionalist posture. That is, deciding which criteria will serve as guides to categorise phenotypic belonging rather underlines the socio-cultural processes by which these conventions come to be.

It seems to me that the doctors of the Medical Commission did not make this distinction between sex differentiation and sex expression seriously. That is, they used a marker of sexual differentiation (Barr corpuscle) as one of sexual expression, which necessarily placed the women they were studying in the category of "abnormality". Instead, I argue, thinking within the paradigm of sex expression may not lead to the same results, since the latter is predicated on the recognition of multiple factors coming into this expression and the further recognition that we hand-pic one or a couple of them to affirm an individual's sex for pragmatic reasons.

As far as the innovative role of the IOC with regards to the history of gender testing, let us remind the reader that it was the first organisation to use chromatin tests in order to universalize the tests, respect the athletes modesty, and work faster. This attention to efficiency and universal tests may be explained by the size of the operation they had to run. The choice of tests can be described as a molecularization of gender as Gaudillière puts it. In this respect, the IOC was indeed an innovator. Something that continues today with their project of elaborating an athlete's biological passport. The IOC was not the first organisation to give out femininity certificates, but they systematized their use such that an athlete who passed the test wouldn't have to pass it again. In sum, the IOC was not a precursor of the tests, but the medical committee did mark a passage in the medical practice.

That is, their interventions are no longer tied to healing practices. If the motive to "heal" the "poor creatures" is present in 1968, it is quick to take the back seat in later debates. This transition from healing to recommending also rings the bell of a new era in which the biomedical sciences are looked up to weigh in on social decisions. If the hygienists of the 19th century made the connection between their research and political goals very explicit, doctors of the mid 20th

century seem more removed from political decisions while still authoritatively weighing- in on socio-political questions. The emphasis on prevention, present as early as the hygienists as we saw in Chapter 1, is all the more present in sport since it has become the preventative measure by excellence. As such, the healing role of doctors is extended to prevention and hence to "how to lead a good life". This stretch onto the realm of the good life easily leads itself to elevating them as experts on social questions. Sports medicine, on the other hand, has the additional characteristic of being interested in how to push the boundaries of the human body ever further, which makes the IOC's medical commission even more prone to innovation, both in terms of treatment and testing.

The years 1968 to 1972 are years within which the gender tests took the shape they would keep for the next 30 years, at least in terms of procedure within the IOC: i.e. universal, non-invasive tests. There was a change from chromatin tests to chromosomal (SRY gene) tests in the 80's, through polymerase chain reaction, but this pertained to the technique more than the procedural and philosophical aspects of the tests. Hence, for thirty years, the tests took place more or less in the same way as was decided by the early members of the medical committee. They were innovators in so far as the medical certificates, used by the IAAF as of 1946, outsourced their ethical and medical questions because those had to be issued by individual doctors of the athlete's country. Whereas the IOC gave itself the role to resolve the matter by itself. This taking charge of the matter, however, also requires the mobilisation of outside experts as is seen by the reports written by Drs. Vazquez and Van Den Berghe, whom weren't members of the MC. But we also saw, in the case of the Danish doctors, that this gave the MC the liberty to pick which experts they listened to. Wrynn indeed notes that "outside individuals and agencies were consulted, but were not included as part of the decision-making process in a substantive manner" (Wrynn 2004:215).

The Medical Commission, however, were not pioneers in terms of their understanding of sex/gender. Indeed, as seen by the report of these same Danish doctors, and despite their effort to question the status quo, the MC had a pretty traditional understanding of sex/gender. They recognized that biological sex was difficult to determine but nonetheless decided to proceed with their dimorphic conception.

As for the participation of women within their ranks, the IOC also seems to lag behind in this period. Unlike other industries or companies whose work force was affected by men going to the front during the world wars, the IOC was able to pause during the war and hence not rely on the work of women. This is not the case today. It could be argued that the IOC today is ahead of many industry in terms of gender parity and also in terms of the importance they give to research into that subject (Comité international olympique 2012; Pape 2017; Women's Sports Foundation 2017). Between 1968 and 1972, however, this was not the case.

Because of the change in vocabulary used for the post- 2009 policies, later commissions may appear to be less interested in defining was counts as women or men. The policies no longer have to do with "femininity" but with "hyperandrogenia" or "DSD". The impact, however, is still the same as 1967 : gatekeep who can participate in women's sports. As a result, a certain level of androgens is already considered not feminine. That is not to say there haven't been any change since 1967. The change of vocabulary is not just cosmetic. The IOC is no longer focused on what counts as a woman (even if the result is to shape what a woman is), but discusses what is fair towards other athletes. That reflexion would be useful were it applied to all athletes, men included. We may decide to move towards categories according to testosterone levels, like boxing is divided by weight (in men, there are 10 weight categories at the OG and 4 for women) such that framing the question of fairness in terms of gendered competitive categories would be pointless.

The double objective of the IOC's MC at its foundation is, as we saw earlier, to determine sex as well as to give the IOC anti-doping strategies and guidelines. Anabolic steroids, however, were not placed on the banned list until 1976, despite their well-known performance enhancing and masculinizing effects. Steroids, were, apparently, too difficult to detect to be tested. It is rumoured that the Germans were using anabolic steroids in the 1930 to boost their athletic performance. I am inclined to believe this given some previous archival research I did on the use of steroids in concentration camps. German Drs. would have been familiar with the effects of testosterone. I could not, however, find confirmation of this. In any case, as of the 1950s, the soviet union and east German state started using steroids on their athletes of both sexes. That fueled the suspicions about their athletes. Ironically, the same year the tests started, an east-

German IOC medical representative was presenting a project to use doping nationwide to increase performances.

We also saw in the minutes of the 6th meeting that there is a strong push back against alcohol tests while noticing that no federations were fighting against gender testing. Another interesting aspect is that each federations (or many of them) already had their methods to test for doping, which in this case, creates a conflict as to how to standardise the method. Whereas for gender testing, I believe it has only been previously conducted in England by the AF.

In the same minutes, we learn a lot on the state of the knowledge at the time on anabolic steroids and hormones. Perhaps the research had been largely dropped after the second world war and Steinach's failure? The fact that the hormonoïde were allowed in 1968 pretty much answers my question as to why they did not proceed with hormone level testing right off the bat and went for the Barr test. It also somewhat contradicts the earlier stated goals of sports as upholding the "purity" of the athletes. Why is the long-range secondary effects of the drugs relevant? Is the idea not to keep the purity of the athletes?

In the same meeting, denouncement is also strongly condemned, which is interesting because this is pretty much what ends up happening to Semenya in 2009.

We saw that section 6 of Meeting 1 concerning Dr. Génin's comment on the legality of the tests seems to imply that the IOC will not meet any legal difficulty concerning the tests because participation in the OG is made on a voluntary basis. The logic, therefore, is that the IOC is not forcing their tests onto any athletes. In theory, the athlete has the choice not to participate if they wish to avoid the tests. This theme will come back later with the "dissuading motives". Hence, the fact that someone could not participate to the OG because they are neither a woman or a man according to the IOC's categories is not seen as a limitation of someone's rights. Whereas, later, the Commission is careful to discuss that no women should be discriminated against by being refused admission to the games (see the executive committee minutes and also "risque d'atteinte aux droits de la personnalité humaine" in Dr. Thiébault's report of July 1968(Medical Commission 1968a Annexe IV p.4)). This is also interesting in relation to the concerns IOC Vice President, Judge Kéba Mbaye, brought forth in 1988. He posited "two primary concerns, one orienting around fundamental human rights—that is, one cannot be forced to provide proof of

one's own guilt— and the other in relation to health—the principle of the inviolability of a person's physical integrity" (Henne 2009:64). This shift in legal interpretation seems like an interesting avenue to explore for future research.

We also see that it is the refusal of each countries' definition of sex (and, consequently, of their certificates) which gives the Commission members the impression that they must test all athletes. The quest is therefore for a universal standard for gender, no regional variation is admitted.

In Chapter 3, we discussed feminist epistemologies. One could ask to what extent these epistemologies played into this research. My answer is that feminist epistemologist's interest in androcentric biases oriented my choice of topic and set of questions. That is, the category of the "Death of the subject" influenced how I understand knowledge creation. Further, it seems to me this epistemological paradigm would answer the question of the impact of testosterone on the physical advantage of an athlete in the following way: testosterone alone should not be the determining factor in establishing the advantage of an athlete over another. The diffractive method suggests that many other factors need to be taken into account. This is point is made by Camporesi and Maugeri in "Unfair advantage and the myth of the level playing field in the IAAF and IOC policies on hyperandrogenism: when is it fair to be a women?". There, they make the point that there are two types of advantages: 'performance' and 'property' advantage. The former, they write, is a relationship of superiority between performance numbers possessed by different athletes (or teams) and is defined as follows: "A has a final performance advantage over B if A has a better final performance number than B". The latter is defined as "A has an advantage over B in property X if A has a more favourable amount of this property X than B does", where properties are "constituent parts of competitors and competition environment." One of the examples of property advantages offered by Hämäläinen is the oxygen-carrying property possessed by Finnish cross-country skier Eero Mäntyranta, who won two Gold medals in cross-country skiing at the 1964 Winter Olympics in Innsbruck. This genetic mutation is characterized by an elevated absolute red blood cell mass and a consequent increase of 25-50% in the blood oxygen carrying capacity. However, a property advantage alone does not result in a performance

advantage, as other factors contribute to excellence in competition (Montanola and Olivesi 2016, 51).

Another point this epistemological overview allows to underline is the relation with the historical periods in feminism. In the 1968-72 period, feminists contestation regarding the construction of knowledge was not so strong. We saw that the feminist epistemologists only emerged in the 1970s and reached they peak in the 1990s, when the tests were suspended. Further research would be needed to determine whether feminist activism had an impact on the suspension of the tests. The fact that no women were part of the commission before 1972 is also worth noting. Perhaps welcoming women into the commission allowed for a different outlook on the tests. Further research would help elucidate that.

On the other hand, the epistemologies present in the texts might suggest the presence of androcentric bias. The mere fact that an advantage is automatically ascribed to men without the shadow of a doubt is telling. Indeed, language such as "the weaker sex" (Medical Commission 1968a, 1, Annexe II), women being of "short stature" and having "short limbs" are a few of the obvious biases. Apart from that the epistemology present in the text are very much empiricist one, and to a lesser extent, after 1996, feminist empiricism. It is possible that doctors, as scientists, believed in the polymorphous quality of the body but that in their role of medical commission, they were forced to take up another paradigm. Indeed, Jean-Paul Gaudillière argues, in *La médecine et les sciences*, that the biomedical sciences can be separated into three functions: experimental, clinical, and social (Gaudillère 2006, 20). The first is where specimens are analyzed and models constructed (based on observations of animals and laboratory experiments hence made in isolation from the world. The second, clinical, is where diagnostics are elaborated and punctual administration of care is given. The third is the biomedical sciences of the statistical surveys and risk preventions – it is the science of the hygienists, concerned with the population that troubles the states' tranquility. If we understand those functions as roles, we can begin to see that the Medical Commission's roles in the typologies elaborated concerning their relationship to the body. That is, their relationship has been qualified as one where the MC sees the body as a "site of intervention", a "scientific object" and an "object of ethical concern". Site of intervention corresponds to clinical. Scientific object corresponds to the experimental function.

And the third, social function, corresponds to “object of ethical concern” . Each of those relationship to the body requires, in turn, a different conception of the body. Indeed, if doctors see their role in relation to the body as one of providing care to fix ailments, the classification into the normal and pathological or abnormal becomes the prime task. If the role is, on the other hand, an experimental one, the body is more easily apprehended as a scientific object and there is therefore more space for a polymorphic paradigm to emerge. Finally, if the function or role of the scientist is one that is concerned with the population that troubles the states’ tranquility, the interest the doctor has for the body becomes an ethical interest and the dimorphic apprehension of the body is more easily explained. Indeed, the interest in category formation and organization of the social to insure order.

Conclusion

In this thesis, we saw, in Chapter 1, that the biomedical sciences have used different categories for the body through time. There, we came across three categories for discussing phenotypic expression (that can be divided into seven sub-categories): anatomical, hormonal, and chromosomal sex. Sexual determination had been ascribed two causes: gonadic (and, by extension, hormonal) and genetic. We also saw that distinguishing between sex determination and expression was important. Lastly, we saw that the social sciences have proposed different ways of understanding sexual phenotypes: through the paradigms of isomorphism and incommensurability. Further, the social sciences have proposed three ways of conceptualizing the relationship between biological sex and social sex (gender). Sex as being equal to gender, gender as being a sign of the sex and sex being the sign of gender.

In Chapter 2, we saw that women were long discouraged from practicing sports in as high an intensity as men. When they were indeed allowed or encouraged to practice sport, it was so they could give birth to healthier babies, preferably male ones, who could defend the nation. They officially competed in the Olympics for the first time at the Paris OG in 1900 but they remained mostly unwelcomed and limited in their participation until the late 1960s. It seems that it has been their inclusion into the track and field events that triggered the first suspicions of "fraudulent" participation in 1932, but that the first test took place in 1936. These tests were "anatomical tests" which were followed by "chromosomic" and then hormonal tests. To this day, it is still the track and field events that garner the most controversy with the new policies targeting specifically five events in this discipline (400-meters, 400-meters hurdles, 800-meters, hammer throw, pole vault) in which it is said that statistically significant advantages for women with higher testosterone levels are present.

In Chapter 3, I presented three categories of response to androgenic bias proposed by the feminist epistemologists: changing the subject, multiplying the subject, and killing the subject. We saw that the category of killing the subject gathered three strategies: postmodernist accounts of difference, New Materialism, and psychodynamism of individuation. I suggested that the

strategy of psychodynamism of individuation could help resolve some tension found in New Materialism. This resolution of the tension I have called relational ontology. We saw in Chapter 4 that this conception was akin to the methods of Grounded Theories, which is why I chose content analysis as a method.

In the analysis of the meeting minutes of the IOC's Medical Commission between 1967 and 1972, we saw three semantics on the body and three semantics on the relationship between those bodies and the doctors. Namely, bodies are discussed as polymorphic, considered normal when easily categorized in one of the two body morphisms, and abnormal when not. These bodies are further discussed in terms of their relationship to doctors. These relationships are that of the bodies as scientific objects, as abstracted categories, and as objects of ethical concern.

These categories highlight some tensions within the Medical Commission's discourse on the body and some ethical concerns related to its' relation to the bodies it seeks to protect and treat. Indeed, with its current regulations, the IOC is not only determining what counts as a women today (deciding that the bodies of women do not produce over 5nmol/ liter of testosterone, when, based on the empirical evidence, they do), but, doing so, they are also shaping the women of tomorrow. Male athletes who face no upper testosterone limit are encouraged to take hormones if their testosterone levels are under 10nmol/liter (when, arguably, it is likely that their bodies simply produce less testosterone and therefore the IOC should accept that certain men produce less testosterone). Who is to say that men's upper limit has not changed with time and increased exercise? It is impossible to know the trajectory that women's body in relationship to their testosterone levels may take. Especially so if women are forced to limit their testosterone production.

Indeed, the current hypothesis about the causal relationship between testosterone and muscle building is that the former enables the latter. Consider for an instant, however, the reverse hypothesis, i.e. that intense exercise and muscle building is what produces testosterone[138]. This hypothesis actually seems to fit with our current Darwinist theories. If that hypothesis turned out

[138] In fact, as I am correcting this section, a study published on September 12 in Cell Metabolism, on the relationship between bones and hormonal production, goes in the direction of this hypothesis (J. M. Berger et al. 2019)

to be true, women would actually be prevented by the IOC from reaching their full natural potential (i.e. adapt to their environment, here, elite sport).

Besides this, the women targeted by the IOC, i.e. with hyperandrogenism, are women. By refusing to count them as such, the IOC is pre-emptively creating their own categories, refusing to adjust them according to empirical evidence (creating additional categories instead of revisiting their notions about the categories they have created themselves) amounts to a self-fulfilling prophecy which will doubtless serve to further stabilize the differences between the sexes. Does this mean that we ought not to have categories based on "biological sex"? Answering this question is beyond the scope of this study.

What this study showed, however, is that the medical Commission's efforts to establish once and for all a definition of sex that would transcend all the local disputable definitions, or that would allow a firm grip of the truth in order to prevent being "tricked" by opponents or other nations, has been arduous at best. The imperative to settle such a complicated question, to find a common consensus, is commendable. One may indeed argue that some form of consensus has to be found in order to move forward. What I hope this study has showed, however, is that this scientific representation of sex and the decision pertaining to which definition to put forth do not merely rely on "pure" and "disembodied" objectivity.

To both of the previous points one can argue that science is a process and, as such, some questions, on the one hand, simply cannot be answered by it for now. This processual nature implies, on the other hand, that there are some things it offers answers to which will eventually be contested and modified. That is not a bad thing. It is why science, as a method, works: it incorporates new data and adjusts to changing empirical evidence. It does mean, however, that we need not rely on it alone to make decisions. Especially when those decisions affect people's lives so thoroughly. Therefore, an alternative seems to be to try to find common ground elsewhere. Before addressing this alternative, let us consider another ethical question this processual nature of science brings forth in this specific case.

In chapter 2, on the history of the tests, we saw that Helen Stephens was the initial source of concern or suspicion of Avery Brundage who imagined that some male athletes were parading

as women to steal records. Helen turned out to satisfy the Commission's definition of a "woman" at the time (police or visual examination). One may wonder, because of this processual nature and the correspondingly changing nature of the gender tests, if she would satisfy the current definition of the IOC, now that they are able to scrutinize athletes' hormone levels. I wish not so much to speculate about Stephens' biology, but rather to underline the problem of finding scientific responses to social problems: in this specific case, there seems to be a moving goalpost about what it means to be a woman.

To come back to the alternative way to find common ground then, I would like to suggest that approaching these questions from a pragmatic point of view may rid us of many of the above-mentioned problems. Indeed, much more important than what counts as a woman or a man is what it is that we (or, here, the IOC) wants to regulate with gender or doping tests. This is much easier to answer. A hypothesis from my research so far is that the IOC wants to keep its athletes safe by limiting their exposition to drugs whose consumption is dangerous or of which the long-term consequences are unknown. Indeed, the crucial question is how to move forward, together, in the face of uncertainty. To address this, allow me to close with a story which is dear to me and which exposes a pragmatic response to this question.

In 1913, Otto Neurath offers a solution to the Descartes' parable of the lost wanders. This parable expresses an old paradox about rationality. The paradox goes like this: say someone strives to act rationally at all times. Imagine, too, that this person finds herself in a situation in which there is no obvious reason to prefer one option over another. This person, if she follows her own maxim of greatest rationality, will be paralyzed into indecision, *paradoxically* acting in a most irrational way. Descartes illustrates this paradox with the parable of the lost wanders. He writes:

> My second maxim was to be as firm and determined in my actions as I could be, and not to act on the most doubtful decisions, once I had made them, any less resolutely than on the most certain. In this matter I patterned my behavior on that of travelers, who, finding themselves lost in a forest, must not wander about, now turning this way, now that, and still less should remain in one place, but should go as straight as they can in the direction they first select and not change the direction except for the strongest reasons. By this method, even if the direction was chosen at random, they will presumably arrive at some destination, not perhaps where they would like to be,

> but at least where they will be better off than in the middle of the forest. Similarly, situations in life often permit no delay; and when we cannot determine the course which is certainly best, we must follow the one which is probably the best; and when we cannot determine even that, we must nevertheless select one and follow it thereafter as though it were certainly best (Descartes [1637] 2009).

Neurath expands on this parable by assigning each traveler a hypothetical motive for picking one direction over another. He writes:

> One of them is driven in some direction by instinct, another by an omen; the third will carefully consider all eventualities, weigh all arguments and counter-arguments and, on the basis of inadequate premises of whose deficiencies he is unaware, he will in the end, his, head lifted in pride, take one definite direction which he considers the correct one. The fourth, finally, will think as well as he can, but not refrain from admitting that his insight is too weak, and quietly allow himself to decide by lot (Neurath [1913] 1983, 10).

In Neurath's story, the third traveller has no more prestige or authority than the first, second, or fourth. In fact, the fourth traveller is the hero of his story. Indeed, unlike the third traveller, Neurath's "pseudo-rationalist", he recognizes the limitations of his own reason and relies on what Neurath calls an auxiliary motive. Now, this auxiliary motive is all well and good when one "deciding" alone, but what happens when we need to decide as a group and convince others to hop on the same train to collectively move forward (in any direction)? Neurath, for all his insight about the auxiliary motive and despite mentioning three provisional moral rules at the beginning of his paper[139], does not come back on the possible connection that can be made between moral rules, or values, and auxiliary motives.

It seems to me that feminist epistemologists, such as Harding, when they propose an epistemology centered around democratic values, are building on this possible connection. They are saying rationality cannot be what we rely on to decide collectively – especially amid so much uncertainty. Though they do more than this, in my opinion. Indeed, they give values, such as democracy, an epistemological status, claiming that more democratic knowledge practices confer insights that are closer to the truth. Though I tend to sympathise with this epistemological premise, my proposition is more timid. I do not propose democratic knowledge practices on the

[139] "One should adapt oneself to the usual laws, customs and religious views; act energetically even if insight is insufficient; and change oneself rather than the order of the world" (Neurath 1983:3).

basis that it confers better insights (though it may!). Rather, I propose it on the basis of some auxiliary motive in the face of uncertainty. If this auxiliary motive does not lead to insights that are closer to the truth, it will at least lead to research and policy that are closer to the interests of those affected by those policies.

In the case of the IOC, this may look like a widespread consultation of the various actors involved: coaches, athletes, IOC members, etc. To come back to the question of what it is that we, or the IOC, is trying to do with the sex or doping tests, some 60 female athletes have already voiced their opposition to the current IAAF policy on gender testing. They argued that "no woman should be required to change her body to compete in women's sport". If the intention of the IOC is to protect its athletes, then this would compatible with this goal and an example of a democratically voted upon auxiliary motive from whence to develop further policy.

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Annexes

Annex I – Documents offered at the IOC Library

Cote	Titre	Dates	Code contenant
LISTE DES DOSSIERS D'ARCHIVES			
CIO JO-J928W-MEDIC	Contrôle médical lors des Jeux Olympiques d'hiver de St-Moritz 1928 : correspondance	1927.01.01-1928.12.31	C-J02-1928/2
CIO JQ-1936W-MEDIC	Médecine du sport aux Jeux Olympiques d'hiver de Garmisch-Partenkirchen 1936 : brochure commémorative	1936.01.01-1936.12.31	GF 10
CIO JO-1956W-MEDIC	Plan du service sanitaire de la commission sanitaire des Jeux Olympiques d'hiver de Cortina d'Ampezzo 1956	1956.01.01-1956.12.31	C-J02-1956/4
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (U· Y)	1968.01.01-1968.12.31	C-J01-1968/13
CIO JO-1968S-MEDIC	Effets de l'altitude élevée de la ville de Mexico sur les performances des athlètes : correspondance et rapports	1964.01.01-1967.12.31	C-J01-1968/11
CIO JO-1968S-MEDIC	1er séminaire international pour l'étude (génétique et anthropologique) des athlètes olymplques à Mexico les 17·21 juillet 1967: participants, compte-rendus et rapport	1967.01,01-1967.12.31	C-J01-1968/11
CIO JO-1968S-MEDIC	Dopage aux Jeux Olympiques d'été de Mexico 1968 : résultats des examens médicaux par sport	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Congrès International sur l'étude du sport et Congrès argentin de médecine du sport : Informations, conclusions et recommandations. Etude sur les problèmes psychologiques des grands athlètes :rapport	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (A·E)	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (F-J)	1968.01.01·1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olymplques d'été de Mexico 1968 : authentification des examens par pays (K·T)	1968.01.01-1968.12.31	C-J01-1968/13
CIO JO-1968W-MEDIC	Rapports d'analyse de laboratoire par athlète pour le contrôle de dopage aux Jeux Olympiques d'hiver de Grenoble 1968	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1968W-MEDIC	Rapports d'analyse de laboratoire par sport pour le contrôle de dopage aux Jeux Olympiques d'hiver de Grenoble 1968	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenobles 1968: communiqué de presse et communiqué pour les délégations à propos des contrôles antidopage et de féminité	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1972S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Munich 1972 : dopage et tests de féminité : correspondance, analyses, organisation des contrôles et protestations	1972.01.01-1973.09.29	C-J01-1972/17

CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : résultats d'analyse,correspondance, publication sur le dopage, communiqué de presse et rapports	1971.01.01-1972.12.31	C-J02-1972/6
CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : fiches d'enregistrement et cartes d'instruction pour le contrôle de dopage des athlètes (patinage sur glace, biathlon, ski, hockey sur glace, bobsleigh et luge)	1972.01.01-1972.12.31	C-J02-1972/7
CIO JO-1972W-MEDIC	Congrès international de médecine de sport d'hiver aux Jeux Olympiques d'hiver de Sapporo 1972	1972.01.01-1972.12.31	C-J02-1972/6
CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : graphiques, photos et explications des tests	1972.01.01-1972.12.32	JL 13
CIO JO-1976S-MEDIC	Affaires mécicales aux Jeux Olympiques d'été de Montréal 1976 : rapport médical et rapport scientifique sur les contrôles de dopage	1977.01.01-1978.12 .31	C-J01-1976/29
CIO JO-1976S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Montréal 1976 ; procès-verbal des réunions quotidiennes de la Commission médicale du CIO (24 réunions entre le 10 juillet et le 1er août 1976)	1976.01.01-1976.12.31	C-J01-1976/29
CIO JO-1976S-MEDIC	Affaires médicales aux Jeux Olympiques d'hiver d'lnnsbruck 1976 : correspondance, communiqué de presse et bulletin, rapport sur les services médicaux et dispositions concernant le contrôle du taux d'alcool et formulaires pour le contrôle de dopage	1974.01.01-1976.12.31	C-J02-1976/9
CIO JO-1980S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Moscou 1980 ; correspondance	1977.01.01-1981.12.31	C-J01-1980/38
CIO JO-1980S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Moscou 1980 : procès-verbal des réunions quotidiennes de la Commission médicale du CIO (22 réunions entre le 14 Juillet et le 2 août 1980), conférences de presse et documents d'information	1980.01.01-1980.12.31	C-J01-1980/38
CIO JO-1980S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Moscou 1980 ; contrôle de dopage et contrôle de féminité : correspondance, rapports et formulalres vierges	1978.01.01-1981.12.31	C-J01-1980/39
CIO JO-1980W-MEDIC	Affaires médicales des Jeux Olympiques d'hiver de Lake Placid 1980 : correspondance, formulaires vierges et rapport médical	1976.01.01-1980.12.31	C-J02-1980/24
CIO JO-1980W-MEDIC	Contrôle dopage aux Jeux Olympiques d'hiver de Lake Placld 1980 : analyse des échantillons	1980.01.01-1980.12.31	C-J02-1980/23
CIO JO-1980W-MEDIC-CT	Programme de recherche et de contrôle du dopage aux Jeux Olympiques d'hiver de Lake Placid 1980 ;correspondance et contrat entre le COJO et l'Institut National de la Recherche Scientifique (INRS •Québec)	1977.01.01-1977.12.31	C-J02-1980/24

CIO JO-1984S-MEDIC	Affaires médicales aux Jeux Olymplques d'été de los Angeles 1984 : correspondance, rapports journaliers des soins prodigués et kit d'information sur les services médicaux	1978.01.01-1983.12.31	C-J0l-1984/55
CIO JO-1984S-MEDIC	Affaires m édicales (dopage) aux Jeux Olympiques d'été de Los Angeles 1984 ; correspondance, conférence de presse, formulaires vierges et articles de presse	1980.01.01-1994.12.31	C-J0l-1984/55
CIO JO-1984S-MEDIC	Affaires mêdicales (contrôle de dopage) aux Jeux Olympiques d'été de los Angeles 1984 : organisalion des contrôles de dopage : listes et tableaux par sport et par jour, et correspondance avec les fédérations Internationales (FI)	1983.01.01-1984.12.31	C-J0l-1984/56
CIO JO-1984S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Los Angeles 1984 : rapports et articles de presse	1983.01.01-1984.12.31	C-J01-1984/56
CIO JO-1984S-MEDIC	Affaires médicales (contrôle de dopage) aux Jeux Olympiques d'été de Los Angeles 1984 : correspondance avec les fédérations Internationales (FI)	1983.01.01-1984.12.31	C-J01-1984/55
CIO JO-1984S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Los Angeles 1984 : brochures et publications du COJO	s. d. (sine dato)	JR 69
CIO JO-1984S-MEDIC	Affaires médicales (biomécanique et contrôle de féminité) aux Jeux Olympiques d'été de Los Angeles 1984 : rapport et correspondance	1982.01.01-1985.12.31	C-J0l •1984/56
CIO JO-1984W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sarajevo 1984 : procédure et résultats des analyses des échantillons tests pour l'accréditation du laboratoire de contrôle de dopage (échantillons n•311 à 320)	1984.01.01-1984.12.31	C-J02-1984/16
CIO JO-1984W-MEDIC	Affaires médicales aux Jeux Olympiques d'hiver de Sarajevo 1984 : rapports du COJO sur l'Of'Qanisation des services médicaux, des contrôles de dopage et de1éminité	1982.01.01-1984.12.31	C-J02·1984/16
CIO JO-1984W-MEDIC	Affaires médicales aux Jeux Olympiques d'hiver de Sarajevo 1984 : correspondance	1980.01.01-1984.12.31	C-J02·1984/16
CIO JO-1984W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sarajevo 1984 : résultats des analyses des échantillons tests pour l'accréditation du laboratoire de contrôle de dopage (échantillons n°315 à 320)	1984.01.01-1984.12.31	C-J02-1984/16
CIO JO-1984W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sarajevo 1984 : rapport concernant les contrôles anti-dopage, récapitulatif par jour des soins prodigués aux participants et formulaire vierge pour le contrôle dopage	1984.01.01-1984.12.31	C-J02·1984/17
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenoble 1968: rapports sur les activilés de la commission médicale, sur les contrôles de dopage et de féminité	1968.01.01-1968.12.31	C-J02-1968/16
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenoble 1968 : exemples de chromatogrammes obtenus sur des prélèvements	1968.01.01-1968.12.31	C-J02·1968/9

CIO JO-1968S-MEDIC	Médecine aux Jeux Olympiques d'été de Mexico 1968 et 3e compétition sportive Internationale à Mexico en octobre 1967 : rapports	1967.01.01-1968.12.31	C-J0l-1968/11
CIO JO-1976W·MEDIC	Affaires médicales aux Jeux Olympiques d'hiver d'lnnsbruck 1976: réunions quotidiennes de la commission médicale du CIO : procès-verbaux	1976.01.01-1976.12.31	C-J02-1976/9
CIO JO-1976S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Montréal 1976 : contrôle de féminité et cas de dopage: correspondance, arlides de presse et communiqués de presse	1976.01.01-1976.12.31	C-J0l-1976/28
C-J01-1988/133	Médecine et contrôles de dopage (disqualmcations) lors des Jeux Olympiques d'été de Séoul 1988: correspondance, articles de presse, communiqués de presse, rapports	1988.01.01-1996.12.31	C-J01-1988/28
C-J01-1988/123	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : correspondance	1982.01.01-1987.12.31	C-J01-1988/26
C-J02-1988/135	Médecine (études biomécaniques) aux Jeux Olympiques d'hiver de Galgary 1988: correspondance concernant les éludes et les films biomécaniques	1984.01.01-1989.12.31	C-J02-1988/23
C-J02-1988/187	Médecine (études biomécaniques) aux Jeux Olympiques d'hiver de Galgary 1988: rapports	1986.01.01-1988.12.31	C-J02-1988/40
C-J02-1988/188	Médecine aux Jeux Olympiques d'hiver de Calgary 1988 : correspondance, rapports, réunion des médecins d'équipe et assurance	1982.01.01-1989.12.31	C-J02-1988/32
C-J02-1988/189	Médecine (contrôles de dopage et de féminité) aux Jeux Olympiques d'hiver de Calgary 1988 : correspondance, listes des substances dopantes, formulaire vierge, communiqués de presse et procédure	1983.01.01-1988.12.31	C-J02-1988/33
C-J02-1988/190	Médecine (laboratoire) aux Jeux Olymplques d'hiver de Calgary 1988: correspondance et rapport	1987.01.01-1988.12.31	C-J02-1988/32
C-J01-1988/124	Médecine el contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : correspondance	1988.01.01-1990.12.31	C-J01-1988/26
C-J01-1988/125	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988: correspondance avec les médecins d'équipe	1988.01.01-1988.12.31	C-J01-1988/26
C-J01-1988/126	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : correspondance avec les FI (par sport: A-H)	1986.01.01-1991.12.31	C-J01-1988/27
C-J01-1988/127	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : correspondance avec les FI (par sport: J-V)	1986.01.01-1988.12.31	C-J01-1988/27
C-J01-1988/128	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : formulaires vierges, horaires des contr ôle s, liste des coordinateurs FI et guide des procédures	1988.01.01•1988.12.31	C-J01-1988/27

C-J01-1988/129	Médecine el contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : rapports quotidiens du laboratoire	1988.09.1 B-1988.10.02	C-J01-1988/27
C-J01-1988/130	Médecine et contrôles de dopage aux Jeux Olympiques d'été de Séoul 1988 : rapport sur les infrastructures et services médicaux,services et &s1edes réunions de la COl'TVTlission médicale et conférence sur les activités du laboratoire	1988.01.01-1989.12.31	C-J01-1988/27
C-J02•1992/097	Médecine aux Jeux Olympiques d'hiver d'Albertville 1992 : correspondance(y compris les contrôles de féminité)	1987.01.01-1993.12.31	C-J02-1992/18
C-J02·1992/098	Médecine aux Jeux Olympiques d'hiver d'Albertvlil e 1992 : revue de presse, projet d'assistance médicale et rapport de préparation	1989.01.01-1992.12.31	C-J02-1992/18
C•J02·1992/099	Médecine eux Jeux Olympiques d'hiver d'Albertvine 1992 : rapport d'activité du laboratoire entl•dopage, rapport sur les tests de féminité et rapport sur le service médical	1992.01.01-1992.12.31	C-J02-1992/18
C·J02·1992/204	Médecine aux Jeux Olympiquesd'hiver d'AlbertviUe 1992: formulaires	1991.12.01-1992.12.31	C-J02-1992/18
C-J01-1992/349	Médecine (contrôles de dopage) aux Jeux Olympiques d'été de Barcelone 1992 : correspondance	1990.01.01-1991.12.31	C-J01•1992/75
C-J01-1992/350	Médecine (contrôles de dopage) aux Jeux Olympiquesd'été de Barcelone 1992 : correspondance	1992.01.01-1993.12.31	C-J01·1992/75
C-J01-1992/352	Médecine (contrôles de féminité) aux Jeux Olympiques d'été de Barcelone 1992 : correspondance	1988.01.01-1993.12.31	C·J01•1992/75
C-J01-1992/353	Médecine (études biomécaniques) aux Jeux Olympiques d'été de Barcelone 1992 : correspondance et présentation des projets	1989.01.01•1994.12.31	C-J01•1992/76
C-J01-1992/354	Médecine (études biomécan iques) aux Jeux Olympiques d'été de Barcelone 1992: rapport sur la natation (projet 1O)	1995.01.01-1995.12.31	C-J01•1992/76
C-J01-1992/355	Médecine (congrès scientifique) aux Jeux Olympiques d'été de Barcelone 1992 : programme préliminaire et rapport final	1991.01.01-1992.12.31	C-J01•1992/75
C-J01-1992/356	Médecine (Jeux Sans Fumée) aux Jeux Olympiques d'été de Barcelone 1992: correspondance et publication	1988.01.01-1992.12.31	C-J01-1992175
C-J01-1992/357	Médecine (laboratoire) aux Jeux Olymplques d'été de Barcelone 1992 : correspondance, présentation et rapport d'activité	1988.01.01-1992.12.31	C-JOH992/76
C-J01-1992/358	Médecine aux Jeux Olympiques d'été de Barcelone 1992 : correspondance, planification des services médicaux et visite d'Inspection	1989.01.01-1992.12.31	C-J01-1992/76
C-J01-1992/359	Médecine aux Jeux Olympiques d'été de Barcelone 1992 : rapports	1992.01.01-1992.12.31	C-JOH992/77
C-J01-1992/490	Médecine (études biomécaniques) aux Jeux Olympiques d'été de Barcelone 1992: liste des projets (1· 14)	1992.01.01-1992.12.31	A3 34

C-J01-1992/491	Médecine (Jeux Sans Fumée) aux Jeux Olympiques d'été de Barcelone 1992: accord entre le président du COJO et le président de la commission de coordination	1989.01.01-1989.12.31	JL18
C-J02-1994/205	Médecine et contrôles de dopage aux Jeux Olympiques d'hiver de Lillehammer 1994 : correspondance, communiquées et articles de presse	1989-1996	C-J02-1994/37
C-J02-1994/206	Médecine et contrôles de dopage aux Jeux Olympiques d'hiver de Lillehammer 1994 : règles, manuel de procédure, guide médical, formulaire pharmaceutique et fiches de contrôle vierges	1993-1993	C-J02-1994/37
C-J01-1988/297	Médecine et contrôles de dopage (cas positifs) aux Jeux Olympiques d'été de Séoul 1988: rapport de l'audience de R. Pound par la commission d'enquête contre les pratiques interdites et usage des drogues concernant l'athlète Ben Johnson	1989.01.01-1989.12.31	C-JOH 988/56
C-J01-1996/307	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : correspondance générale	1990.01.01-1997.12.31	C-J01-1996/78
C-J01-1996/308	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : rapports d'avancement à la Commission médical du CIO	1994.02.01-1994.02.28	C-J01-1996/78
C-J01-1996/309	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : rapports d'avancement à la Commission médical du CIO	1995.09.01-1995.09.30	C-J01-1996/78
C-J01-1996/310	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : nominations et accréditations	1990.01.01-1996.12.31	C-J01-1996/79
C-J01-1996/311	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : bureaux et réunions de la Commission médicale	1994.01.01-1996.12.31	C-J01-1996/79
C-J01-1996/312	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : bureaux et réunions de la Commission médicale	1995.01.01-1996.12.31	C-J01-1996/79
C-J01-1996/313	Médecine (laboratoires) aux Jeux Olympiques d'été d'Atlanta 1996: accréditation du laboratoire SmithKline Beecham	1993.01.01-1996.12.31	C-J01-1996/79
C-J01-1996/314	Médecine (laboratoires) aux Jeux Olympiques d'été d'Atlanta 1996: laboratoire SmlthKllne Beecham au Morehouse School of Medicine : accord, informations et rapport final	1994.01.01-1997.12.31	C-J01-1996/80
C-J01-1996/315	Médecine (laboratoires) aux Jeux Olympiques d'été d'Atlanta 1996: correspondance concernant les contrats avec les laboratoires	1996.01.01-1997.12.31	C-J01-1996/80
C-J01-1996/316	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : médicaments et suivi médical au Village	1990.01.01-1997.12.31	C-J01-1996/80
C-J01-1996/317	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 ; Medical Controls Guide	1995.01.01-1996.12.31	C-J0l-1996/82
C-J01-1996/318	Médecine (contrôles de féminité) aux Jeux Olympiques d'été d Atlanta 1996 : correspondance, Informationset accord	1994.01.01-1996.12.31	C-J0l-1996/82

C-J01-1996/319	Médecine (contrôles de dopage) aux Jeux Olympiques d'été d'Atlanta 1996 : correspondance	1995.06.01-1998.12.31	C-J0l-1996/82
C-J01-1996/320	Médecine (contrôles de dopage) aux Jeux Olympiques d'été d'Atlanta 1996 : manuel des opérations des contrôles de dopage	1996.06.01-1996.06.30	C-J0l-1996/83
C-J01-1996/321	Médecine (contrôles de dopage) aux Jeux Olympiques d'été d'Atlanta 1996 : 1-![gh Aesolution Mass §pectrometry (HAMS) : correspondance et rapport	1996.01.01-1997.12.31	C-J0l-1996/83
C-J01-1996/322	Médecine (contrôles de dopage) aux Jeux Olympiques d'été d'Atlanta 1996 : formulaires, sécurité des échantillons, ratio T/E, articles de presse, vidéo	1995.01.01-1998.12.31	C-J0l-1996/83
C-J01-1996/323	Médecine (contrôles de dopage) aux Jeux Olympiques d'été d'Atlanta 1996 : propositions des FI	1995.01.0H 998.12.31	C-J0l-1996/83
C-J01-1996/326	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : packages de documentation concernant des échantillons numérotés	1996.01.01·1996.12.31	C-J01-1996/84
C-J01-1996/327	Médecine aux Jeux Olym piques d'été d'Atlanta 1996 : packages de documenta tion concernant des échantillons numérotés	1996.01.01-1997.12.31	C-J01-1996/84
C-J01-1996/328	Médecine (cas du brornantane) aux Jeux Olympiques d'été d'Atlanta 1996 : articles, lnfonnations el correspondance	1996.01.01-1996.12.31	C-J01-1996/84
C-J01-1996/329	Médecine (cas du bromantane) aux Jeux Olympiques d'été d'Atlanta 1996: procès TAS concernant les athlètes Andrei Komeev et Zakhar Goullev : transcriptions des audiences du 29 Jui ll et et 3 1 Jul Jet 1996	1996.01.01-1996.12.31	C-J01-1996/84
C-J01-1996/330	Médecine (cas du bromantane) aux Jeux Olympiques d'été d'Atlanta 1996: procès TAS concernant les athlètes Andrei Komeev et,t akhar Goullev : transcriptions de l'audience du 2 aoüt 1996	1996.01.01-1996.12.31	C-J01-1996/85
C-J01-1996/331	Médecine (cas du brornantane) aux Jeux Olympiques d'été d'Atlanta 1996 : proces TAS concernan t les athlètes Andrei Komeev et Zakhar Gouliev: appel, correspondance, sentence final, notes et communiqué de presse	1996.01.01-1997.12.31	C-J01-1996/85
C-J01-1996/332	Médecine aux Jeux Olympiques d'été d'Atlanta 1996: Jeux sans tabac, Prix olympique pour les recherches scientifiques, chiropratique, communiqués de presse, notes des réunions	1994.01.01-1996.12.31	C-J01-1996/85
C-J01-1996/333	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : notes et rapports après les Jeux	1996.01.01-1997.12.31	C-J01-1996/85
C-J01-1996/334	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : rapports Journaliers des Services médicaux	1996.07.19-1996.07.24	C-J01-1996/81
C-J01-1996/335	Médecine aux Jeux Olympiques d'été d'Atlanta 1996 : rapports journaliers des Services médicaux	1996.07.25-1996.07.30	C-J01-1996/81

C-J01-1996/336	Médecine aux Jeux Olymptques d'été d'Atlanta 1996 : rapports joumallers des Services médicaux	1996.07.31-1996.08.03	C-J01-1996/81
B-ID04-MEDIC/001	Commission mécflcale du ClO : correspondance	1960.01.01-1973.12.31	B-ID04-MEDIC/1
B·ID04·MEDIC/002	Commission mécficale du CIO : correspondance	1974.01.01-1979.12.31	B-ID04-MEDIC/1
B-ID04-MEDIC/003	Commission médicale du CIO : correspondance	1980.01.01-1981.12.31	B-ID04-MEDIC/2
B-1D04-MEDIC/004	Commission médicale du CIO : correspondance	1982.01.01-1983.12.31	B-ID04-MEDIC/2
B-ID04-MEDIC/005	Commission médicale du CIO : correspondance	1984.01.01-1985.05.31	B-ID04-MEDIC/2
B-ID04-MEDIC/006	Commission médicale du CIO : correspondance	1985.06.01-1986.03.31	B-ID04-MEDIC/2
B-ID04-MEDIC/007	Commission médicale du CIO : correspondance	1986.04.01-1987.02.28	B-ID04-MEDIC/3
B-ID04-MEDIC/008	Commission médicale du CIO : correspondance	1987.03.01-1987.12.31	B-ID04-MEDIC/3
B-ID04-MEDIC/010	Commission médicale du ClO : relations avec le Conseil de l'Europe et son Comité de l'Education extrascolaire : correspondance, avant-projet de convention et documents sur le dopage des athlètes	1962.01.01-1986.12.31	B-ID04-MEDIC/4
B-ID04-MEDIC/011	Commission mécficale du ClO : relations avec Organisation des Nations Unies (ONU) et son Conseil économique el social : notes et rapport sur l'abus des stupéfiants	1967.01.01-1968.12.31	B-ID04-MEDIC/3
B-ID04-MEDIC/012	Commission médicale du CIO : correspondance concernant les jeux régionaux	1974.01.01·1987.12.31	B-ID04-MEDIC/4
B-ID04-MEDIC/013	Commission médicale du CIO : relations avec I' "International Association of Olympie Medical Officers" (IAOMO) : correspondance, constitutions, actes du 2e congrès mondial et liste des participants à la 1ère session	1971.01.01-1987.12.31	B-ID04-MEDIC/4
B-ID04-MEDIC/014	Commission médicale du CIO : Association olympique internationale pour la recherche rnédico-sportive (AOIRMS) : correspondance	1982.01.01•1988.12.31	B-ID04-MEDIC/5
B-ID04-MEDIC/015	Commission médicale du CIO : Association olympique Internationale pour la recherche médco-sportive (AOIRMS): contrats avec ISL Marketing et Kistler, emblèmes, projets, publlcalions el statuts	1982.01.01-1984.12.31	B-ID04-MEDIC/5
B-ID04-MEDIC/016	Commission médicale du CIO : Association olympique Internationale pour la recherche médco-sportive (AOlRMS): réunions à Lausanne le 8 décembre 1982, le 16 février 1983 et le 14 janvier 1985: liste de présence, procès-verbal et documents de travail	1982.01.01-1985.12.31	B-ID04-MEDIC/5
B-ID04-MEDIC/017	Commission médicale du CIO : textes sur ses activités, fonctions et décisions	1961.01.01-1983.12.31	B-ID04-MEDIC/5
B-ID04-MEDIC/018	Commission médicale du CIO : articles de presse, articles spécialisés et rapports	1968.01.01-1986.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/019	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1963.01.01-1968.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/020	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance	1969.01.01-1974.12.31	B-ID04-MEDIC/6

B-ID04-MEDIC/021	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance	1975.01.01-1977.12.31	B-ID04-MEDIC/7
B-ID04-MEDIC/022	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance	1978.01.01-1979.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/023	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance	1980.01.01-1982.12.31	B-ID04-MEDIC/7
B-ID04-MEOIC/024	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1983.01.01-1983,10.31	B-ID04-MEDIC/7
B-ID04-MEDIC/025	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1983.11.01-1984.06.30	B-ID04-MEDIC/8
B-ID04-MEDIC/026	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1984.07.01-1985.12.31	B-ID04-MEDIC/8
B-ID04-MEDIC/027	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1986.01.01-1986.12.31	B-ID04-MEDIC/8
B-ID04-MEDIC/028	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1987.01.01-1987.12.31	B-ID04-MEDIC/9
B-ID04-MEDIC/030	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance et articles de presse	1965.01.01-1988.12.31	B-ID04-MEDIC/9
B-ID04-MEDIC/031	Commission médicale du CIO : dopage et substances dopantes : articles spécialisés, études et rapports	1962.01.01-1985.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/032	Commission médicale du CIO : dopage et substances dopantes: étude critique	1964.01.01-1964.12.31	GF 77
B-ID04-MEDIC/033	Commission médicale du CIO : symposium sur le contrôle de dopage des athlètes à Moscou le 9 octobre 1979 et stage sur laJytl_!contre te dopage à Cologie tes 25	1979.01.01-1983.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/034	Commission médicale du CIO : dopage et contrôle de féminité : correspondance	1967.01.01-1973.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/035	Commission médicale du CIO : contrôle de féminité : correspondance	1936.01.01-1982.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/036	Commission médicale du CIO : contrôle de féminité : correspondance	1983.01.01-1987.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/037	Commission médicale du CIO : contrôle de féminité : correspondance, articles de presse, études et analyses	1974.01.01-1988.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/038	Commission médicale du CIO : réunions à Lausanne les 26-27 septembre, le 20 décembre 1967 et les 13-14 juillet 1968 : liste de Présence, procès-verbal et documents de travail	1967.01.01-1968.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/039	Commission médicale du CIO : réunions à Mexico tes 1er et 12 octobre 1968, à Lausanne les 25-26 janvier 1969, à Münich le 12 juin 1970 et le 29 juillet 1971 : llsle de présence, rapport el procès-verbal	1968.01.01-1971.12.31	B-ID04-MEDIC/11

B-ID04-MEDIC/040	Commission médicale du CIO : réunions à Sapporo les 29-30 janvier et 3 février 1972, à Münich les 21, 23, 25-26 août et 1er-2 septembre 1972, et à Moscou les 26-27 mai 1973 : liste de présence, procès- verbal et documents de travail	1971.01.01-1973.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/041	Commission médicale du CIO : réunions à Innsbruck les 5-7 avril 1974 et à Montréal les 23-25 avril 1975 : procès-verbal et documents de travail	1972.01.01-1975.12.31	B-ID04-MEDIC/12
B-ID04-MEDIC/042	Commfssl on médicale du CIO : réunions à Innsbruck les 30 janvier-15 février 1976, à Montréal les 10.12 juillet et -1er aout 1976 et à Barcelone les 14-15 octobre 1976: procès-verbla, documents de travail et correspondance	1974 .01.01-1976.12.31	B-ID04-MEDIC/12
B-ID04-MEDIC/043	Commission médicale du CIO: réunion de la sous-commission scientifique à Bruxelles les 19-20 mars 1977 : procès-verbal	1977.01.01-1977.12.31	B-ID04-MEDIC/13
B-ID04-MEDIC/044	Commission médicale du CIO: réunion à Lausame les 7-8 mai 1977: liste de présence, procès-verbal et documents de travail	1976.01.01-1977.12.31	B-ID04-MEDIC/12
B-ID04-MEDIC/045	Commission médicale du CIO: réunion à Lake Placld les 17·19 juin 1978: listes de présence, procès- verbat, documents de travail et transcription	1978.01.01-1978.12.31	B-ID04-MEDIC/12
B-ID04-MEDIC/046	Commission médicale du CIO: réunions à Moscou les 8·12 octobre 1979, à Lake Placid les 7·23 février 1980 et à Moscou les 19 juillet•23 août 1980: listes de présence, procès-verbal, compte-rendus et documents de travail	1978.01.01-1980.12.31	B-ID04-MEDIC/13
B-ID04-MEDIC/047	Commission médicale du CIO: réunions à Baden-Baden le 24 septembre 1981 et à Rome les 23·24 mai 1982 : liste de présence, procès-verbal et documents de travail	1981.01.01-1982.12.31	B-ID04-MEDIC/13
B-ID04-MEDIC/048	Commission médicale du CIO : réunions à Sarajevo les 12·14 février 1983 et à Helsinki le 6 août 1983 : liste de présence, procès-verbal et documents de travail	1982.01.01-1983.12.31	B-ID04-MEDIC/13
B-ID04-MEDIC/049	Commission médicale du CIO : réunion à Sarajevo tes 5·19 février 1984 : liste de présence, procès• verbal, dessin d'une séance et documents de travail	1983.01.01-1984.12.31	B-ID04-MEDIC/13
B-ID04-MEDIC/050	Commission médicale du CIO : réunion à Los Angeles les 24 juillet • 12 août 1984 : liste de présence, procès-verbal et documents de travail	1983.01.01-1984.12.31	B-ID04-MEDIC/14
B-ID04-MEDIC/051	Commission médicale du CIO : réunion à Séoul les 20-22 avril 1986 : liste de présence, procès-verbal et documents de travail	1985.01.01-1986.12.31	B-ID04-MEDIC/14
B-ID04-MEDIC/052	Commission médicale du CIO: réunion à Calgary les 24·26 lévrier 1987: liste de présence et procès• verbal	1987.01.01-1987.12.31	B-ID04-MEDIC/14
B-ID04-MEDIC/053	Commission médicale du CIO : réunion à Calgary les 24·26 février 1987 : documents de travail	1986.01.01-1987.12.31	B-ID04-MEDIC/15
B-ID04-MEDIC/054	Commission médicale du CIO : réunion à Calgary les Q.28 février 1988 : liste de présence, procès- verbal et documents de travail	1987.01.01-1988.12.31	B-ID04-MEDIC/15
B-ID04-MEDIC/055	Commission médicale du CIO : réunion à Séoul les 14 septembre • 1er octobre 1988 : procès-verbal et documents de travail	1987.01.01•1988.12.31	B-ID04-MEDIC/15

B-ID04-MEDIC/056	Commission médicale du CIO : sous-commission Dopage et blochlmle du sport : correspondance	1980.01.01-1985.12.31	B-ID04-MEDIC/16
B-ID04-MEDIC/057	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : correspondance	1986.01.01-1987.06.30	B-ID04-MEDIC/16
B-ID04-MEDIC/058	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : correspondance	1987.07.01-1988.12.31	B-ID04-MEDIC/16
B-ID04-MEDIC/059	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrôle dopage reconnus et accrédités par le CIO : correspondanceconcernant le code d'éthique, conditions, formulaire, questionnaire et réunion avec l'IMF	1981.01.01-1988.12.31	B-ID04-MEDIC/17
B-ID04-MEDIC/060	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrôle dopage reconnus et accrédités par le CIO : laboratoires de Barcelone (Espagne) et Breda (Pays-Bas) : correspondance et rapports d'analyse	1984.01.01-1988.12.31	B-ID04-MEDIC/17
B-ID04-MEDIC/061	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrôle dopage reconnus et accrédités par le CIO : laboratoires de Brisbane (Australîe), Calgary (Canada), Cologne (Allemagne) , Helsinki (Finlande) et Huddinge (Suède) : correspondance	1980.01.01-1988.12.31	B-ID04-MEDIC/17
B-ID04-MEDIC/062	Commission médicale du CIO : sous-commissionDopage et biochimie du sport : laboratoires de contrôledopage reconnus et accréditéspar le CIO : laboratoires de Indianapolis (Etats-Unis), Kreischa (Alemagne de rest),Londres (Grande-Bretagne), Madrid (Espagne) et Macolin (Suisse): correspondance	1980.01.01-1988.12.31	B-ID04-MEDIC/18
B-ID04-MEDIC/063	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrôle dopage reconnus et accréditéspar le CIO: laboratoires Los Angeles(Etats-Unis,)Moscou (URSS) et Nijmegen (Pays-Bas) : correspondance et rapports d'analyse	1980.01.01-1988.12.31	B-ID04-MEDIC/18
B-ID04-MEDIC/064	Commlsslon médicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrôle dopage reconnus el accrédités par le CIO: laboratoires de MontJéal(Canada), Prague (Tchécoslovaquie). Paris (France) et Rome (Italie) : correspondance el rapports d'analyse	1980.01.01-1988.12.31	B-ID04-MEDIC/18
B-ID04-MEDIC/065	Commissionmédicale du CIO : sous-commission Dopage et biochimie du sport : laboratoires de contrae dopage reconnus et accrédités par le CIO : laboratoires d'Oslo (Norvège), Sao Paulo (Brésll) et Sarajevo (Yougoslavie) : correspondance et rapports d'analyse	1983.01.01-1988.12.31	B-ID04-MEDIC/19

B-ID04-MEDIC/066	Commission médicale du CIO : sous-commission Dopage et biochimiedu sport : laboratori es de contrôle dopage reconnus et accrédités par le CIO: laboratoires de Tokyo (Japon) et Zagreb (Yougoslavie) : correspondance el rapports d'analyse	1983.01.01-198.812.31	B-ID04-MEDIC/19
B-ID04-MEDIC/067	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : réunions à Cologne tes 28-29 novembre 1980 et 20-22 mai 1981 ; liste de présence et procès-verbal	1980.01.01·1981.12.31	B-ID04-MEDIC/20
B-ID04-MEDIC/068	Commission médicale du CIO: sous-commisison Dopage el biochimie du sport : réunionsle 17 février 1984, à Cologne les 18-19 février 1985 et à Strasbourg les17-18 novembre 1985: liste de présence, procès-verbalet documents de travail	1984.01.01-1985.12.31	B-ID04-MEDIC/20
B-ID04-MEDIC/069	Commission médicale du CIO : sous-commission Dopage et biochimie du sport : réunion à Rome les 12-13 novembre 1986 : liste de présence, procès-verbal et documents de travail	1986.01.01-19861. 2.31	B-ID04-MEDIC/20
B-ID04-MEDIC/070	Commissionmédicaledu CIO : sous-commission Dopage et biochimie du sport : réunions â Calgary le 23 février 1987 et à Moscou les 28-29 septembre 1987 : liste de présence, procès-verbal et documents de travail	1987.01.0·11987.12.31	B-ID04-MEDIC/19
B-ID04-MEDIC/072	Commissionmédicaledu CIO : sous-commission Biomécanique et physiologie du sport : correspondance	1985.01.01-1985.12.31	B-ID04-MEDIC/21
B-ID04-MEDIC/073	Commissionmédicale du CIO : sous-commission Biomécanique el physiologiedu sport : correspondanceet analyse biomécanique de l'haltérophilielors des championnats du monde de 1985	1985.01.01-1988.12.31	B-ID04-MEDIC/21
B-ID04-MEDIC/074	Commission médicale du CIO : sous-commissionBiomécanique et physiologie du sport : publication, études et textes sur la biomécanique	1985.01.01-1985.12.31	B-ID04-MEDIC/21
B-ID04-MEDIC/075	Commission médicale du CIO : sous-commission Biomécanique et physiologie du sport : réunions à Rome les 23-24 mal 1982, à Cologne le 2 novembre 1982 et le 29 septembre 1983, et à Los Angeles les 12-13 novembre 1983: procès-verbal et documents de travail	1982.01.01-1983.12.31	B-ID04-MEDIC/21
B-ID04-MEDIC/076	Commission médicale du CIO : sous-commission Biomécanique et physiologie du sport : réunions à Cologne le 10 avril 1984, à Bruxelles les 8-9 décembre 1984, et les 30 septembre - 1er octobre 1985 : liste de présence, procès-verbal et documents de travail	1984.01.01-1985.12.31	B-ID04-MEDIC/22

B-ID04-MEDIC/077	Commission médicale du CIO : sous-commission Biomécanique et physiologie du sport : réunions à Lausanne les 3-4 mars 1986 et à Séoul les 17-18 septembre 1986: liste de présence, procès-verbal et documents de travail	1985.01.01•1986.12.31	B·ID04·MEDIC/22
B-ID04-MEDIC/078	Commission médlcale du CIO : sous-commission Biomécanique et physiologie du sport : réunions à Calgary les 28 lévrier-1er mars 1987 et à Colorado Springs les 13-14 occlobre 1987 : liste de présence, procès-verbal et documents de travail	1987.01.01-1987.12.31	B-ID04-MEDIC/22
B-ID04-MEDIC/079	Commission médicale du CIO : sous-commission Médecine du sport et orthopédie, et boxe : correspondance	1976.01.01-1988.12.31	B-ID04-MEDIC/22
B-ID04-MEDIC/080	Commission médicale du CIO : sous-commission Médecine du sport et orthopédie, et boxe : articles de presse	1982.01.01-1995.12.31	B-ID04-MEDIC/23
B-ID04-MEDIC/081	Commission médicale du CIO : sous-commission Médecine du sport et orthopédie, et boxe : études et articles sur la boxe et ses effets sur la santé	1977.01.01-1994.12.31	B-ID04-MEDIC/23
B-ID04-MEDIC/082	Commission médicale du CIO : sous-commission Médecine du sport et orthopédie, et boxe : bibliographie, Informations concernant des congrès et symposiums, rapports, recommandations pour améliorer les règles de prévention et règlements sur la boxe	1963.01.01-1991.12.31	B-ID04-MEDIC/23
B-ID04-MEDIC/083	Commission médicale du CIO : sous-commission Médecine du sport et orthopédie, et boxe : réunions à Cologne le 28 septembre 1983 et à Sarajevo le 16 février 1984 : procès-verbal	1983.01.01-1984.12.31	B-ID04-MEDIC/29
B-ID04-MEDIC/100	Commission médicale du CIO : sous-commission Coordination avec les CNO : correspondance	1981.01.01-1988.12.31	B-ID04-MEDIC/28
B-ID04-MEDIC/084	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1985 au Cameroun, en 1986 en Colombie et Corée du sud : correspondance et rapports	1985.01.01-1987.12.31	B-ID04-MEDIC/24
B-ID04-MEDIC/085	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1986 au Costa Rica, Equateur, Guatemala et Irlande : correspondance et rapports	1986.01.01-1988.12.31	B-ID04-MEDIC/24
B-ID04-MEDIC/086	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1986 aux Fidji et Koweit : correspondance et rapports	1985.01.01-1987.12.31	B-ID04-MEDIC/24
B-ID04-MEDIC/087	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1986 au Niger, Pays-Bas, Sierra Leone et Uruguay : correspondance et rapports	1986.01.01-1987.12.31	B-ID04-MEDIC/25

B-ID04-MEDIC/088	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1986 au Chili et en 1987 au Zimbabwe : correspondance et rapports	1986.01.01-1987.12.31	GF 77
B-ID04-MEDIC/090	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1987 au Brésil, Etats-Unis et Japon : correspondance et rapports	1986.01.01-1988.12.31	B-ID04-MEDIC/25
B-ID04-MEDIC/091	Commission médicate du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1987 au Kenya, Norvège, Nouvelle-Calédonie et Papouasie-Nouvelle-Guinée: correspondance et rapports	1986.01.01·1988.12.31	B-ID04-MEDIC/26
B-ID04-MEDIC/092	Commission médicate du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1987 au Sénégal, Somalie et Suisse : correspondance et rapports	1986.01.OH 988.12.31	B-ID04-MEDIC/26
B-ID04-MEDIC/093	Commission médicale du CIO : sous-commission Coordinanon avec les CNO : stages en médecine sportive en 1987 en Tchécoslovaquie et en 1988 en Albanie, Allemagne et Chine : correspondance et rapports	1986.01.01·1988.12.31	B-ID04-MEDIC/26
B-ID04-MEDIC/094	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1987 en Indonésie et Samoa Occidentales : correspondance et rapports	1986.01.0M 988.12.31	GF 78
B-ID04-MEDIC/096	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1988 à Chypre, Congo, Côte d'Ivoire et El Salvador : correspondance et rapports	1987.01.01-1988.12.13	B-ID04-MEDIC/27
B-ID04-MEDIC/097	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1988 aux Etats-Unis, Guam, Inde, Islande, Nouvelle-	1987.01.01-1988.12.31	B-ID04-MEDIC/27
B-ID04-MEDIC/098	Commission médicale du CIO : sous-commission Coordination avec les CNO : stages en médecine sportive en 1988 en Pologne, Portugal, Samoa américaines, Suriname et Swaziland: correspondance et rapports	1987.01.01·1988.12.31	B-ID04-MEDIC/27
B-ID04-MEDIC/099	Commission médicale du CIO : sous-commisslon Coordination avec les CNO : stages en médecine sportive en 1989 au Canada, Corée du sud, Egypte, Emirats arables unis, Finlande, Grande-Bretagne, Guam, Mauritanie, Roumanie, Tonga, Trinité-et-Tobago, Libéra el Turquie : correspondance	1987.01.01·1988.12.31	B-ID04-MEDIC/28
B-ID04-MEDIC/101	Commission médicale du CIO : sous-commission Coordination avec les CNO : réunion à Lausanne les 28·30 octobre 1985 : &ste de présence, procès-verbal et documents de travail	1985.01.01-1985.12.31	B-ID04-MEDIC/28

B-ID04-MEDIC/102	Commission médicale du C10 : sous-commission Coordination avec les CNO : réunion à Lausanne les 2·3 avril 1986 : liste de présence, procès-verbal et documents de travail	1985.01.01-1986.12.31	B-ID04-MEDIC/28
B-ID04-MEDIC/133	Commlsslon médicale du CIO : sous-commission Coordination avec tes CNO : réunion informelle à Brisbane te 25 septembre 1986 : procès-verbal	1986.01.01-1986.12.31	B-ID04-MEDIC/27
B-ID04-MEDIC/103	Commission médicale du CIO : sous-commission Coordination avec tes CNO : réunion à Calgary le 27 février 1987 : Liste de présence, procès-verbal et documents de travail	1986.01.01-1987.12.31	B-ID04-MEDIC/29
B-ID04-MEDIC/104	Commisskm médicale du C10 : sous-commission Coordination avec les CNO : réunion à Lausanne le 9 novembre 1987 : liste de présence, procès-verbal et documents de travail	1987.01.01-1987.12.31	B-ID04-MEDIC/29
B-ID04-MEDIC/105	Commission médicale du CIO: sous-commission Coordination avec les CNO : réunion à Calgary le 12 février 1988 : Uste de présence, procès-verbal et documents de travail	1987.01.01-1988 .12.31	B-ID04-MEDIC/29
B-ID04-MEDIC/108	Commission médicale du CIO : réunion des 2 sous-commissions Dopage et biochimie du sport et Médecine sportive et orthopédie à Los Angeles les 6•7 février 1982: liste de présence, procès-verbal et documents de travail	1981.01.01-1982.12.31	B-ID04-MEDIC/30
B-ID04-MEDIC/109	Commission médicale du CIO : réunion des 2 sous-commissions Dopage et biochimie du sport et Médecine sportive et orthopédie à Cologne le 29 septembre 1983 : procès-verbal	1983.01.01-1983.12.31	B-ID04-MEDIC/30
B-ID04-MEDIC/110	Commission médicale du CIO : réunion des 3 sous-commissions Dopage et biochimie du sport, Médecine sportive et orthopédie et Coordination avec les CNO à Moscou les 10-11 avril 1985 : liste de présence, procès-verbal et documents de travail	1984.01.01-1985.12.31	B-ID04-MEDIC/30
B-ID04-MEDIC/145	Commission médicale du CIO: question du dopage (avant la création de la commission) : correspondance, texte et rapports	1937.01.01-1938.12.31	B-ID04-MEDIC/30

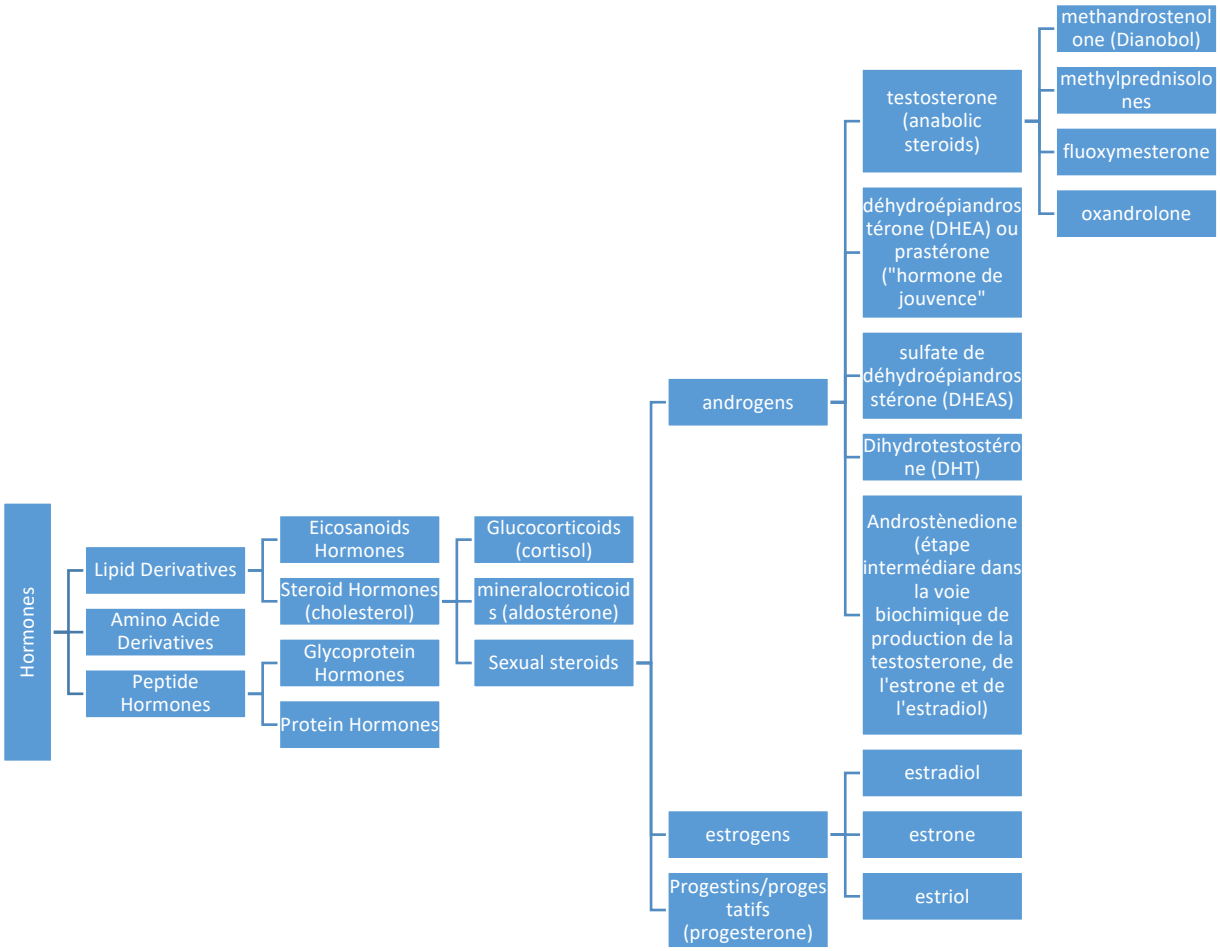
Annex II — Documents Consulted at the IOC Library

Of the 211 files made available to me at the IOC in July 2017, I took out 35. Of these, I used 28 based on my assessment of their relevance to my research.

Cote	Titre	Dates	Code contenant
B-ID04-MEDIC/031	Commission médicale du CIO : dopage et substances dopantes : articles spécialisés, études et rapports	1962.01.01-1985.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/032	Commission médicale du CIO : dopage et substances dopantes: étude critique	1964.01.01-1964.12.31	GF 77
B-ID04-MEDIC/034	Commission médicale du CIO : dopage et contrôle de féminité : correspondance	1967.01.01-1973.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/035	Commission médicale du CIO : contrôle de féminité : correspondance	1936.01.01-1982.12.31	B-ID04-MEDIC/10
B-ID04-MEDIC/038	Commission médicale du CIO : réunions à Lausanne les 26-27 septembre, le 20 décembre 1967 et les 13-14 juillet 1968 : liste de Présence, procès-verbal et documents de travail	1967.01.01-1968.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/039	Commission médicale du CIO : réunions à Mexico tes 1er et 12 octobre 1968, à Lausanne les 25-26 janvier 1969, à Münich le 12 juin 1970 et le 29 juillet 1971 : llsle de présence, rapport el procès-verbal	1968.01.01-1971.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/040	Commission médicale du CIO : réunions à Sapporo les 29-30 janvier et 3 février 1972, à Münich les 21, 23, 25-26 août et 1er-2 septembre 1972, et à Moscou les 26-27 mai 1973 : liste de présence, procès- verbal et documents de travail	1971.01.01-1973.12.31	B-ID04-MEDIC/11
B-ID04-MEDIC/145	Commission médicale du CIO: question du dopage (avant la création de la commission) : correspondance, texte et rapports	1937.01.01-1938.12.31	B-ID04-MEDIC/30

Cote	Titre	Dates	Code contenant
CIO JO-J928W-MEDIC	Contrôle médical lors des Jeux Olympiques d'hiver de St-Moritz 1928 : correspondance	1927.01.01-1928.12.31	C-J02-1928/2
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (U· Y)	1968.01.01-1968.12.31	C-J01-1968/13
CIO JO-1968S-MEDIC	Effets de l'altitude élevée de la ville de Mexico sur les performances des athlètes : correspondance et rapports	1964.01.01-1967.12.31	C-J01-1968/11
CIO JO-1968S-MEDIC	1er séminaire international pour l'étude (génétique et anthropologique) des athlètes olymplques à Mexico les 17·21 juillet 1967: participants, compte-rendus et rapport	1967.01,01-1967.12.31	C-J01-1968/11
CIO JO-1968S-MEDIC	Dopage aux Jeux Olympiques d'été de Mexico 1968 : résultats des examens médicaux par sport	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Congrès International sur l'étude du sport et Congrès argentin de médecine du sport : Informations, conclusions et recommandations. Etude sur les problèmes psychologiques des grands athlètes :rapport	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (A·E)	1968.01.01-1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olympiques d'été de Mexico 1968 : authentification des examens par pays (F-J)	1968.01.01·1968.12.31	C-J01-1968/12
CIO JO-1968S-MEDIC	Contrôle antidopage aux Jeux Olymplques d'été de Mexico 1968 : authentification des examens par pays (K·T)	1968.01.01-1968.12.31	C-J01-1968/13
CIO JO-1968W-MEDIC	Rapports d'analyse de laboratoire par athlète pour le contrôle de dopage aux Jeux Olympiques d'hiver de Grenoble 1968	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1968W-MEDIC	Rapports d'analyse de laboratoire par sport pour le contrôle de dopage aux Jeux Olympiques d'hiver de Grenoble 1968	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenobles 1968: communiqué de presse et communiqué pour les délégations à propos des contrôles antidopage et de féminité	1968.01.01-1968.12.31	C-J02-1968/9
CIO JO-1972S-MEDIC	Affaires médicales aux Jeux Olympiques d'été de Munich 1972 : dopage et tests de féminité : correspondance, analyses, organisation des contrôles et protestations	1972.01.01-1973.09.29	C-J01-1972/17
CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : résultats d'analyse,correspondance, publication sur le dopage, communiqué de presse et rapports	1971.01.01-1972.12.31	C-J02-1972/6
CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : fiches d'enregistrement et cartes d'instruction pour le contrôle de dopage des athlètes (patinage sur glace, biathlon, ski, hockey sur glace, bobsleigh et luge)	1972.01.01-1972.12.31	C-J02-1972/7
CIO JO-1972W-MEDIC	Dopage aux Jeux Olympiques d'hiver de Sapporo 1972 : graphiques, photos et explications des tests	1972.01.01-1972.12.32	JL 13
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenoble 1968: rapports sur les activilés de la commission médicale, sur les contrôles de dopage et de féminité	1968.01.01-1968.12.31	C-J02-1968/16
CIO JO-1968W-MEDIC	Médecine aux Jeux Olympiques d'hiver de Grenoble 1968 : exemples de chromatogrammes obtenus sur des prélèvements	1968.01.01-1968.12.31	C-J02·1968/9
CIO JO-1968S-MEDIC	Médecine aux Jeux Olympiques d'été de Mexico 1968 et 3e compétition sportive Internationale à Mexico en octobre 1967 : rapports	1967.01.01-1968.12.31	C-J0l-1968/11
B-ID04-MEDIC/001	Commission mécflcale du ClO : correspondance	1960.01.01-1973.12.31	B-ID04-MEDIC/1
B-ID04-MEDIC/010	Commission médicale du CIO : relations avec le Conseil de l'Europe et son Comité de l'Education extrascolaire : correspondance, avant-projet de convention et documents sur le dopage des athlètes	1962.01.01-1986.12.31	B-ID04-MEDIC/4
B-ID04-MEDIC/011	Commission mécficale du CIO : relations avec Organisation des Nations Unies (ONU) et son Conseil économique el social : notes et rapport sur l'abus des stupéfiants	1967.01.01-1968.12.31	B-ID04-MEDIC/3
B-ID04-MEDIC/017	Commission médicale du CIO : textes sur ses activités, fonctions et décisions	1961.01.01-1983.12.31	B-ID04-MEDIC/5
B-ID04-MEDIC/018	Commission médicale du CIO : articles de presse, articles spécialisés et rapports	1968.01.01-1986.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/019	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes: correspondance	1963.01.01-1968.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/020	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance	1969.01.01-1974.12.31	B-ID04-MEDIC/6
B-ID04-MEDIC/030	Commission médicale du CIO : dopage, contrôle de dopage et substances dopantes : correspondance et articles de presse	1965.01.01-1988.12.31	B-ID04-MEDIC/9

Annex III – Some Notions on Hormones



Annex IV – Meeting Attendances

Name	Country	Speciality	Sep-67	Dec-67	Jul-68	Oct-68	Oct-68	Jan-69	Jun-70	Jul-71	Jan-72	Jan-72	Feb-72	Aug-72	Aug-72	Aug-72	Aug-72	Sep-72	Sep-72	attendance
	where		Lausanne	Lausanne	Lausanne	Mexico	Mexico	Lausanne	Munich	Lausanr	Sapporo	Sapporo	Sapporo	Munich	Munich	Munich	Munich	Munich	Munich	
Alexandre de Mérode	Belgium		present	present	present	present	present	present	present	present	present	present	present	present	present	present	present	present	present	17
Prof. Giuseppe La Cava	Italy		present	absent	present	N/M	present	present	present	N/M	N/M	present	N/M	present	present	present	present	present	present	12
Dr. Roger Genin	France		present	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	2
Prof. Dr. Ludwig Prokop	Austria	Doping expert, Pr. Physiology a	present	present	Present	present	present	present	present	present	present	present	present	present	present	present	present	present	present	17
Dr. Albert Dirix	Belgium	Doping	present	present	present	present	N/M	present	N/M	N/M	present	present	present	present	present	present	present	present	present	14
Dr. Eduardo Hay	Mexico	Genetics, Gynaecology, Obstet	present	present	present	present	present	present	N/M	N/M	present	N/M	present	present	present	present	present	present	present	14
Dr. P. van Dijk	Netherlands		present	absent	present	present	present	present	present	N/M	N/M	N/M	N/M	present	present	present	present	present	present	12
Prof. Arnold H. Beckett	England	Doping	present	present	present	N/M	present	present	N/M	N/M	present	present	present	present	present	present	present	present	present	14
Dr. Alex Vecheler	Swisserland		present	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	2
M.J.W. Westerhoff			present	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	2
M. Arpad Csanadi	Hongry		absent	present	present	present	present	present	N/M	present	present	present	N/M	present	present	present	present	present	present	14
General Reinderhoff			N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Yoshio Kuroda	Japan		N/M	N/M	absent	present	present	absent	N/M	N/M	N/M	present	present	present	present	present	present	present	present	10
Dr. Herbert Reindell	Germany		N/M	N/M	present	present	absent	present	present	N/M	N/M	N/M	N/M	present	present	present	present	present	present	10
Dr. Jacques Thiébault	France		N/M	N/M	present	present	present	N/M	present	N/M	present	present	present	present	present	present	present	present	present	13
Mr. W. Daume	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Mr. H. Kunxe	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Hein	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Käfer	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	2
Mr. Knoesel	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Hegels	Germany		N/M	N/M	N/M	N/M	N/M	N/M	present	present	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	3
Dr. Donicke			N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Hauck			N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	2
Mr. Kroppenstedt			N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Mr. A. Takac			N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Daniel P. Hanley	USA		N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	present	present	present	present	present	present	present	present	9
Dr. Nina Grashinskaia	USSR		N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	present	present	present	present	present	present	present	present	9
Amilcare Rotta			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
M. Leclef			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Bert Isatisch			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dan Steler			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Dedic			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
H. Roos			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. Raas Ernst	Austria		N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
J. Hederen			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	1
Dr. C. Laurin	Canada		N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	present	present	present	present	present	6
Colonel Grut			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	1
Pr. Montanaro			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	1
Pr. Schonholzer			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	N/M	N/M	1
Pr. Kaarlo Hartiala			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	N/M	1
Mr. Jones			N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	1
Mr. Rieckehoffs	Puerto Rico		N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	N/M	present	1
			10	10	10	9	9	9	15	6	17	10	9	14	14	18	14	16	16	206
																			average	12.12
																			mode	10
																			median	10
																			highest	18
																			lowest	6

Annex V – Translation of table 1 in Making Sex Revisited Dekonstruktion Des Geschlechts Aus Biologisch-Medizinischer Perspektive Voß's (2010)

Chapter III, pp. 277–81

Tab. 1: Überblick über die häufig für Geschlechtsdetermination als wichtig beschriebenen Gene bzw. Genprodukte. (Die Tabelle stellt dabei nur einige, nämlich die hier zuvor betrachteten, Gene dar und ist nicht als vollständige Gesamtübersicht zu verstehen.)

Tab. 1: Overview of genes or gene products that are often considered important for sex determinations. (The table represents only a few, namely the genes considered here [in this chapter] and is not to be understood as a complete overview.) My note: This list presents 21 genes that are involved in the process of sex determination and adds that the list is not exhaustive. The common conception that SRY is like a switch that lights on masculinity or femininity is therefore quite far from the truth.

Gen Gene	Genort (human) Locus	Humane Proteinfunktion (murin in Klammern) Human protein function (mice in parentheses)	Datenbank-Eintrag, vgl. OMIM (2008) Database entry, cf. OMIM (2008)
Exprimierte Gene in der undifferenzierten geschlechtlichen Anlage Genes expressed in an undifferentiated sexual system			
WT1		Transkriptionsfaktoren, wobei mehr als zwei Dutzend verschiedene Isoformen bekannt sind, die unterschiedliche Wirkungen entfalten; WT1 ist ggf. an der Regulation der Transkription von SRY, AMH und SF1 beteiligt; zudem könnte es wichtig bei der Ausbildung der Nierenfunktion, bei der Myogenese und ggf. bei der Ausbildung der Netzhaut sein (Maus: Ggf. ist WT1-Protein auch an der Regulation von Wnt4 und Dax1 beteiligt; ggf. stabilisiert es posttranskriptional Sry mRNA; ggf. unterschiedliche Wirkung der Isoformen von Maus und Mensch) Transcription factors, where more than two dozen different isoforms are known to produce different effects; WT1 may be involved in the regulation of transcription of SRY, AMH and SF1; moreover, it might be important in the formation of kidney function, in the case of myogenesis and possibly in the formation of the retina (Mouse: Wt1 protein may also be involved in the regulation of Wnt4 and Dax1, possibly stabilizing post-translationally SRY mRNA, possibly different effect of isoforms of mouse and human)	607102
SF1		Kernrezeptor / Transkriptionsfaktor, SF1 wird eine Bedeutung bei der Ausbildung der indifferenten Keimdrüse zugeschrieben; außerdem sei es beteiligt an der Regulation von DAX1, CYP11A1, STAR, P450 Aromatase, von Hydroxylasen und Gonadropinen; insbesondere bewirke es die Expression von AMH und die Bildung von Testosteron (Maus: Zunächst in allen steroidbildenden Geweben exprimiert, gehe die Expression von Sf1 bei XX-Chromosomen nach 12,5 dpc zurück) Nuclear receptor / transcription factor, SF1 is attributed a significance in the formation of the indifferent gonad; besides, it is involved in the regulation of DAX1, CYP11A1, STAR, P450 aromatase, of hydroxylases and	184757

		gonadropins; in particular it causes the expression of AMH and the formation of testosterone (mouse: initially expressed in all steroid-forming tissues, the expression of Sf1 in XX chromosomes goes back to 12.5 dpc)	
LHX9		Transkriptionsfaktor, genauere Untersuchungen stehen aus (Maus: Beteiligung an der Ausbildung von Hoden und Eierstöcken zugeschrieben; ggf. an der Regulation der Transkription von SF1 beteiligt) Transcription factor, more detailed investigations are needed (mouse: participation in training attributed to testes and ovaries; possibly involved in the regulation of transcription of SF1)	606066
LIM1 (LHX1)		(Maus: Transkriptionsfaktor; Bedeutung wird bei der Ausbildung von Kopfstrukturen sowie von Nieren und Genitalfurche zugeschrieben) (Mice: transcription factor, meaningful in the formation of head structures, kidney and genital furrow)	
EMX2		Transkriptionsfaktor, genauere Untersuchungen stehen aus (Maus: Einflüsse bei der Ausbildung des Zentralen Nervensystems und bei der Entwicklung des Urogenitaltraktes zugeschrieben; Mäuse ohne funktionales EMX2-Protein bildeten weder Keimdrüsen noch Genitaltrakt aus) Transcription factor, more detailed investigations are needed (mouse: influences attributed to the formation of the central nervous system and in the development of the genito-urinary tract, mice without functional EMX2 protein did not form gonads or genital tract)	600035
M33		Transkriptionsfaktor, genauere Untersuchungen stehen aus (Maus: Einfluss sowohl bei Hoden- als auch bei Eierstockentwicklung, daher wird angenommen, dass M33 upstream von SRY wirke) Transcription factor, more detailed investigations are needed (mouse: influence in both testicular as well also in ovarian development, it is therefore assumed that M33 acts upstream of SRY)	602770
Gene, die als an einem ‚hodendeterminierenden Signalweg' beteiligt beschrieben werden. Genes that are described as involved in Testis Determination [TDF]			
GATA4/ggf.mit dem co-Faktor FOG2		(insbesondere Maus: Transkriptionsfaktor, Expression in der indifferenten Keimdrüse; anschließend ggf. Aktivierung der Transkription von Sry und von Amh	600576 (FOG2: 603693)

		und damit Bedeutung bei der Hodenentwicklung [dies ggf. zusammen mit FOG2]; ggf. zusammen mit FOG2 Bedeutung bei der Eierstockentwicklung; zudem Bedeutung bei der Entwicklung des Herzens und bei der Ausbildung von Ovarialfollikeln beschrieben) (In some mouse: transcription factor, expression in the indifferent gonadal, then possibly activation of the transcription of SRY and Amh and thus meaningful in the development of testicles [this possibly together with FOG2], possibly together with FOG2 importance in ovarian development; also important in the development of the heart and in described the formation of ovarian follicles)	
SRY		Transkriptionsfaktor, als bedeutsam bei der Ausbildung von Hoden beschrieben Transcription factor, described as significant in the formation of testicles	480000
SOX9		Transkriptionsfaktor, ggf. Spleißfaktor; bedeutsam bei der Ausbildung von Skelett und Bindegewebe und bei der Ausbildung von Hoden, ggf. auch bei der Melanin-Produktion; Untersuchungen von Mensch und Maus weisen darauf hin, dass SOX9 hinreichend für die Ausbildung von Hoden und einen als männlich betrachteten Phänotyp ist (dies gelte auch bei Abwesenheit von funktionalem SRY) Transcription factor, possibly splicing factor; important in the formation of skeletal and connective tissue and in the formation of testicles, possibly also in melanin production; Studies of humans and mice indicate that SOX9 is sufficient for the development of testes and a male phenotype (this also applies in the absence of functional SRY).	608160
AMH		Hormon; Bedeutung wird AMH bei der Rückbildung des Müllerschen Ganges zugeschrieben; in späteren Embryonalphasen wird AMH Bedeutung bei der Ausbildung von Ovarialfollikeln zugeschrieben Hormone; Meaning is attributed to AMH in the regression of Müller's Ganges; in later Embryonic phases are attributed to AMH significance in the formation of ovarian follicles	600957
SF1		vgl. Oben	184757

		see above	
FGF9		(Maus: Signalmolekül; Bedeutung in der Embryonalentwicklung sowie in adulten Organismen bei Gewebereparaturmechanismen; Beteiligung an der Ausbildung von Hoden beschrieben) (Mouse: signaling molecule, important in embryonic development as well as in adult organisms in tissue repair mechanisms; production of testicles)	600921
DMRT1		(Maus und verschiedene andere Organismenarten: Transkriptionsfaktor; hohe Expressionsraten von DMRT1- Homologen in den einzelnen Organismenarten werden mit der Ausbildung von Hoden, niedrige Expressionsraten mit der Ausbildung von Eierstöcken in Verbindung gebracht) (Mouse and various other types of organisms: Transcription factor; high expression rates of DMRT1 homologues in the individual organism species are associated with the formation of testes, low expression rates with the formation of ovaries)	
DMRT3		(Maus und verschiedene andere Organismenarten: Transkriptionsfaktor; auf Grund eines ähnlichen Expressionsmusters wie bei DMRT1-Homologen in verschiedenen Organismenarten wird auch für DMRT3-Homologe eine Bedeutung bei der Ausbildung von Hoden postuliert) (Mice and various other types of organisms: transcription factor, due to a similar expression pattern as in DMRT1, the presence of DMRT3 in different organisms is also homologuous to the above mentionned formation of testes)	602424
SOX8		(Maus: Transkriptionsfaktor; die Bedeutung wird in einer Steigerung der Wirkung von SOX9-Protein gesehen; ggf. interagiere auch SOX8-Protein mit Amh und Sf1; exprimiert wird SOX8 in zahlreichen Geweben und Organen) (Mice: transcription factor, meaning increased SOX9 protein activity, possibly also interacting with SOx8 protein with Amh and Sf1, expressing SOX8 in numerous tissues and organs)	605923
ATRX		Helikase, Bedeutung bei der Ausbildung von Hoden zugeschrieben. Helicase, importance in the formation of testicles	300032

DAX1		Kernrezeptoren / Transkriptionsfaktoren; zwei verschiedene Isoformen (DAX1, DAX1A) sind beschrieben; Bedeutung wird bei der Eierstockentwicklung angenommen, es wird aber auch antagonistische und unterstützende Wirkung bei Hodenentwicklung und Spermatogenese beschrieben (Maus: Auch hier wurde antagonistische und unterstützende Wirkung von DAX1 bei der Hodenentwicklung gezeigt, hingegen zeigte sich kein Einfluss auf die Ausbildung von Eierstöcken.) Nuclear receptors / transcription factors; two different isoforms (DAX1, DAX1A) are described. Importance is assumed in ovarian development, but antagonistic and supportive effects in testicular development and spermatogenesis were also observed (mouse: Again, antagonistic and supportive effect of DAX1 in testicular development observed, however, showed no effect on ovarian production.)	300473
Gene, die als an einem ‚eierstockdeterminierenden Signalweg' beteiligt beschrieben werden. Genes described as involved in Ovarian Determination [ODF]			
		vgl. Oben . see above.	300473
WNT4		Signalmolekül; möglicherweise aktiviert WNT4-Protein die Expression von DAX1; angenommen wird, dass WNT4 für die Ausbildung des Müllerschen Ganges notwendig sei, später an der Entwicklung von Eierstöcken mitwirke und antagonistisch zur Ausbildung von Leydig-Zellen wirke (Maus: Neben der möglichen Aktivierung von Dax1 wurde auch die Repression von Fgf9 durch das WNT4-Protein beschrieben.) Signal molecule; possibly WNT4 protein activates the expression of DAX1; It is assumed that WNT4 for the training of Müller tubes is later involved in the development of ovaries and acts antagonistically to the formation of Leydig cells (mouse: In addition to the possible activation of Dax1, repression of Fgf9 by the WNT4-Protein has been observed.)	603490
GATA4-FOG2		vgl. Oben. See above.	600576 (FOG2: 603693)

FOXL2		Transkriptionsfaktor; FOXL2 könnte für die vollständige Eierstockentwicklung bedeutsam sein (Maus: Bei nicht-funktionalem Foxl2- Gen konnte kein Einfluss auf die Eierstockentwicklung festgestellt werden.) Transcription factor; FOXL2 could be important for complete ovarian development (Mouse: In non-functional Foxl2 gene, no influence on ovarian development could be detected.)	605597
RSPO1		Transkriptionsfaktor; entfaltet möglicherweise in Kooperation mit anderen Faktoren antagonistische Wirkung auf die Entwicklung von Hoden (Maus: Rspo1-Gen wird in der frühen Embryonalentwicklung in zahlreichen Geweben und Körperregionen – nicht nur in der Genitalfurche – exprimiert.) Transcription factor; may develop antagonistic effects on the development of testes in cooperation with other factors (mouse: Rspo1 gene is expressed in early embryonic development in numerous tissues and body regions – not only in the genital furrow.)	609595

Voß, Heinz-Jürgen. 2010. "Kapitel III: Geschlechtsdetermination – von ‚dem Hodendeterminierenden Faktor' Hin Zu Modellen Komplex Interagierender Und Kommunizierender Molekularer Komponenten." In *Making Sex Revisited Dekonstruktion Des Geschlechts Aus Biologisch-Medizinischer Perspektive (3., Unveränderte Auflage 2011)*, 1. Aufl. Bielefeld: transcript Verlag. https://doi.org/10.14361/9783839413296-005.